Istituto Italiano per l'Africa e l'Oriente (IsIAO)

The Collapse Melts Down: A Reply to Farmer, Sproat & Witzel
Author(s): Massimo Vidale
Source: *East and West*, Vol. 57, No. 1/4 (December 2007), pp. 333-366
Published by: Istituto Italiano per l'Africa e l'Oriente (IsIAO)
Stable URL: http://www.jstor.org/stable/29757733
Accessed: 30-10-2015 00:38 UTC

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at http://www.jstor.org/page/info/about/policies/terms.jsp

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Istituto Italiano per l'Africa e l'Oriente (IsIAO) is collaborating with JSTOR to digitize, preserve and extend access to *East and West*.

http://www.jstor.org

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

The Collapse Melts Down

A Reply to Farmer, Sproat & Witzel

by Massimo Vidale

Nothing matches the holiness and fascination of accurate and intricate detail. How can you appreciate a castle if you don't cherish all the building blocks, and don't understand the blood, toil, sweat, and tears underlying its construction?

Gould 2003: 47

Introduction

My purpose is to reply to 'The Collapse of the Indus-Script Thesis: The Myth of a Literate Harappan Civilization', by Steve Farmer, Richard Sproat & Michael Witzel, in *Electronic Journal of Vedic Studies* (*EJVS*), 11, 2, 2004, pp. 19-57. I actually think that the Indus script was probably a protohistoric script, somehow conveying the sounds and words of one or more still unidentified languages. Although proofs are obviously lacking (the only demonstration would be a successful translation), this is the most reasonable assumption; and I must confess that I have lived so far rather content with such uncertainty. I do not believe that any of the proposed decipherments, so far, has hit is target; in this field there has been, and there is, a lot of wild imagination building up upon little or nothing. In order to decipher a lost writing system, you have to guess the language, guess the content, and you need relevant contexts on which independently and reasonably test your ideas. So far, there is no evidence that any one of these three conditions has been fulfilled, and, sad but true, guesses remain guesses. Sometimes, the political background (in terms of contemporary issues) of proposed decipherments – or statements about their failures – are so obvious and backward that they do not deserve further attention. But I also think there is still a lot we should do for best exploiting the relics of the Indus writing system, and work for a possible future solution (see Appendix). But Farmer, Sproat & Witzel loudly stated that they have solved the mystery, that the Indus scrip is not writing, and that they can read or interpret part of the signs. I disagree with their arguments and, perhaps more, with the tone and language adopted by the authors. In this reply, I will make ample use of rhetoric, and will use this word, adapting myself (admittedly, with some enjoyment) to their style. I try to explain why their thesis is not acceptable. I am not a linguist, nor a computational expert. Therefore, the authors will forgive my lack of specific experience in their fields. On the other hand, their way of handling archaeological information on the Indus Civilization (my field of expertise) is sometimes so poor, outdated and factious that I feel fully authorized to answer on my own terms. Also, the

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

reader will forgive me for few and short digressions including personal recollections. I am growing older, and I have started to think that things I did or witnessed in the past are more precious than I previously thought.

The Importance of Dead-ends

Should we be scared by this announced collapse? From the first noun in the title of their paper, Farmer, Sproat & Witzel are eager to communicate to us that previous and current views on the Indus script are naive and completely wrong, and that finally, after 130 years of illusion, through their paper we may finally see the truth behind the dark curtains of a dangerous scientific myth. The name of Asko Parpola only surfaces on the next page, but everybody immediately feels that the real goal of their attack is the Finnish decipherment of the Indus script system. I used to freely talk with my students interested in the Indus Civilization, and I know that to some young and would-be scholars the Finnish decipherment, with its one-sided emphasis on proto-Dravidian substratum and its efforts at identifying in the short texts of the Indus seals astrological-religious correlates between divinities' names, sounds and script signs seems worthless. I would also step aside together with those who are not at all convinced of the actual success of the Finnish efforts, and certainly I do not share many of Asko's archaeo-linguistic reconstructions in his recent papers. But let us talk first about science. It grows like some kind of coral or madrepore – there is a central stem, from which secondary branches grow radially. Slowly, some of these developments are pushed aside, where they start starving for lack of light and oxygen, while others on top are more successful, at least since a powerful drift of growth does not push them, in their turn, at the border. The growth is general, and only weak souls might indulge in the practice of evaluating progress after uni-directional paths, reconstructed *ad hoc* downwardly, whose purpose is only self-praising. Asko and his collaborators might well have pursued a wrong path (even if I might not have the competence judge this) but the quantitative search for regularities in the signs sequences and the Dravidian hypothesis have been a quite reasonable effort, that could only be pursued with passion and the required scientific know how. This is what the Russians, and Parpola and his group tried to do: if they had not, we should have to do it now, starting everything again. Failed attempts – if we are actually dealing with one – sometimes might turn out to be more precious than marginally 'correct' results. And everybody owes to Asko Parpola and his co-authors the beautiful volumes of the Corpus, which cost decades of work and which everybody now uses as a foundation for further study.

Talking with at least one of my students and with some senior colleagues I recorded the impression that Steve Farmer's views, abundantly advertised a couple of years ago on the web, at least finally brought some new insights to a field plagued by old and boring misconceptions. I try to show that in this perception there is probably something true, but much wrong. Actually, as I write below, the view of the Indus Civilization that emerges from the paper I discuss is flat, uncritical and hopelessly traditional. Nothing new under the sun! Actually, as we shall see, some of the most relevant views in the paper might well have been subscribed to by the archaeological establishment 50-20 years ago, and certainly with far greater authority.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

Brevity of the Inscriptions

At p. 19, note 2 we read

> [...] the Indus system cannot be categorized as a 'script' even under [...] broad definitions of the term, since the brevity of the inscriptions alone suggests that they were no more capable of performing extensive mnemonic or accounting functions than of systematically encoding speech.

Simply stated, for the authors there is no way that short inscriptions express language. This article of faith is an important point, because at the end of the paper (p. 48) it re-surfaces in a more articulated way. Here the authors lay down five possible conditions for accepting a possible refutation of their ideas, namely *1.* the find of inscriptions including several hundred signs; *2.* inscriptions of at least 50 symbols with random sign duplications; *3.* bilingual inscriptions with double texts of at least 30 signs (curious. why 30 and not 20 or 25?); *4.* independent testing and validation of decipherment rules across significant bodies of Indus inscriptions; *5.* lexical lists of Near Eastern types. With the exception of the obvious condition *4.*, the others strictly depend upon the chance of finding long texts. At this point, the authors could also have included the existence of monumental inscriptions on rock with royal genealogies crossing not less than a dozen generations, or the precise location of the National Library of Mohenjo-Daro. Archaeological evidence, so far, has indicated that such finds are equally improbable. Everybody knows that the Indus script did not work that way ([1]). Still, I see no historical reason for excluding the actual possibility that the Indus Civilization was '[...] the only known literate society of the world, ancient or modern, that did not produce texts of significant length on durable materials' (p. 26).

At p. 23 the authors consider the use of potsherds as a writing medium across different ancient cultures. They assume, again, that the brevity of Indus texts on vessels rules against their scriptural nature, because ancient civilization wrote on potsherds '[...] medium-sized (and often quite long) texts [...]'. Wrong statement. Length of texts has more to do with the scripts' contexts of use than with cultural attitudes; nor the length of a text is linearly correlated to its potential information. If it is true that in Egypt inscriptions on potsherds might have been long, this is not true for classical Greece, reportedly a literate civilization. Here, for example, the specialized function of exiling a citizen was performed by the means of quite short isolated inscriptions reporting the names of the condemned. On Greek ceramics one can encounter even shorter inscriptions, in the range or in the average sign number of the Indus ones, reporting the name of the owner, of the painter or potter, definitions of vessels' form, their market price, and dedications to *amasioi* or lovers. The words *kalos*, *kale* are 5 and 4 characters only, but written on serving and drinking cups in banquets they acquired specific and widely recognized meanings. On early Christian pottery lamps and vessels one may see inscriptions of few characters, sometimes compounded, that, although having strong symbolic meanings, could also be easily read and phonetically pronounced. An inscription of three

([1]) Incidentally: the mysterious donor that offered a reward of 10,000 dollars for a lengthy Indus inscription must be somehow tight-fisted. Come on, it's only one month of salary of a successful American academic. This sum might represent a lot of money in India or Pakistan, but for the expensive living standards of the Western World it looks like a rather miserable offer. She, or he, could have ventured to risk something more.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

characters incised on an Indus shipping jar, although short, might have conveyed as well in its functional contexts substantial information or key instructions. Farmer, Sproat and Witzel do not think like archaeologists, and therefore completely miss, in handling their view of the past, the many fold implications of contextual meanings.

The authors also indulge in defining the Indus signs appearing on pottery as '[...] oversized symbols ([2]) that few *naive observers* would be tempted to classify as "writing" [...]' (my italics) and express the remarkable 'revolutionary' opinion that such signs represented deities to whom the vessels' content were dedicated. What an amazing finding: take the corpus of inscriptions on pottery, and you have a completely listed Indus pantheon. So far, Indus archaeologists were too naive to grasp this. And as there are inscribed black jars made in the core of the Indus valley and shipped to Oman (Tosi 1991; Méry 2000), the authors should even be credited with having discovered the first evidence of trans-oceanic, intercultural sacrifices of human history!

At the end of such discussion of inscribed ceramics, we read that '[...] the evidence overall makes it impossible to credit claims that pottery or potsherds were used by Indus elites to "scribble messages" to one another', ascribing such view to Kenoyer (1998: 71). This quotation is calculatedly biased and factious. Actually, Kenoyer does not speak of potsherds: Farmer & others indirectly suggest that Kenoyer and others believed in the use of potsherds as a medium for writing letters, while if you read the quoted pass Kenoyer quite reasonably, I think, proposes that merchants might have written on vessels belonging to loads and cargoes short messages regarding the vessels' contents or, as he says, protective charms.

This arbitrary, short-sighted view or prejudice against short inscriptions is really hard to understand. The early pictographic accounting tablets from Uruk may be as short as the texts of the Indus seals or those incised on some types of Indus vessels. Associated or not to speech, would the authors deny their status as early writing? We all know that inscriptions on Near Eastern cylinder seals, in the second half of the 3rd millennium B.C., are frequently as long as the Indus ones, and nonetheless the short texts combine themselves with the iconography to convey some important messages about the identity of the bearer. The common assumption that the Indus texts, too, may report a combination of pictographic and syllabic signs, perhaps expressing – together with the standardized animal icons that figure below the inscriptions in the standard steatite stamp seals – a system of social coordinates, attributions or functions and names stands unthreatened, as it remains compatible with the length of the sequences. For me, this hypothesis rests in forced peace but also in full safety in the limbo of what presently cannot be verified.

Writing on Perishable Materials

Given the different materials on which Indus craftspeople left inscriptions (steatite, carnelian, ivory and bone, pottery, stoneware, faience, copper and gold, inlays on wooden

([2]) Like Alice in Wonderland, Farmer, Sproat & Witzel have a serious problem with the size of the characters of Indus script: for them the inscriptions are too short (*passim*), signs are too small (p. 24), or too large (p. 23). Even the 'gigantic' size of the Dholavira board inscriptions is looked upon with implicit suspicion and criticism (p. 36). It would have been easier to state from the beginning what they think should be the appropriate size of respectable protohistoric writing characters.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

boards), that they also wrote on perishable materials is quite likely. Preservation of materials such as wood, bark, leather, cloth, or palm leaves in the Indus Valley (humid because of the monsoons but also because of the rising water tables) is demonstrably very poor ([3]). On the other hand, that Indus people wrote on organic materials long texts, in the light of the inscriptions we know, appears very unlikely. Thus, we may expect, in the future, to discover some short unreadable sequences on perishable media, but probably nothing more substantial. This is all one can confidently say on the subject.

The authors take two and one half pages (pp. 24-26) to review the history of what they call 'The lost manuscript thesis'. They depict a picture where all the scholars of the past were granitically convinced of the existence in the Indus Civilization of large archives of long manuscripts on perishable supports, until some papers by Farmer, Witzel and others, starting from 1998, unexpectedly and bravely challenged this 'thesis' (p. 25) (they also use colourful tones and the usual rhetoric of scientific discovery in an effort at dramatizing and rendering more appealing their text: see the use of expressions such as 'the first direct challenge' at p. 25, and the detective story-like statement that the lack of manuscript production '[...] is sufficient to close the case'). Such a posterior reconstruction might appear attractive to readers not accustomed to Indus archaeology and, above all, evidently gratifies the authors, but the possible existence of such documents was traditionally mentioned in our field as a possibility (and a remote one), and not certainly as an established belief. Students in archaeology are warned, in their first year classes, of the deep inconsistency of arguments *ex-absentia* (if I do not find it, it does not exist), and the commonplace of referring to possible lost manuscripts was more like crossing fingers than real hope.

The authors perhaps were not there, but I will never forget, a few years ago, the five seconds of absolute silence that fell on the audience when Asko Parpola showed at a South Asian Archaeology conference a thick manuscript pile of palm leaves covered with thousands of tiny Indus characters surrounding some Tantric-like designs. Obviously it was a forgery, but a provocative one: somebody in India or Pakistan had tried to fulfil an unavowable archaeological dream, spending months in painstakingly writing nonsense, with the hope of making a huge pile of dollars – a hope frustrated by a simple 14C test. This story alone says that the authors this time are right, when suggesting (at length) the need of dismissing forever this old archaeological commonplace.

On the Concept of 'Literacy' in Early State Societies

The authors have a clear attitude at knocking down walls even where there are readily available unlocked doors. That archaeologists have accepted the Indus script as comparable to the writing systems of Sumer and Egypt for them is '[...] an incontrovertible historical fact'

([3]) I still think that this also explains the rarity of unbaked clay sealings at Mohenjo-Daro, particularly because the site was excavated by picks and shovels. For example, there is not a single excavated fireplace in its houses. As one cannot seriously assume that fireplaces were not used at Mohenjo-daro, we have to assume that, like elevated walls, they were build in clay, and because of their fragility and similarity to the collapsed material in the fillings, such infrastructures did not survive the techniques of excavation of the past.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

(p. 20). After reading their paper, the authors comment with self-praising, many readers would be surprised to discover that '[...] the standard view that the Indus civilization was literate has been an assumption and not a conclusion of previous studies' (see p. 48). The paper surprised me in many points, but, *pace* Farmer (I copy one of the rhetorical expressions of the paper), not certainly because of this. Practically speaking, everybody knows that this view has been and still is an assumption; but for the moment being it is also the prevalent, most likely archaeological hypothesis. But here, too, the authors might have a point: by many of us this hypothesis has always been taken for granted, even because of the lack of better alternatives. Let us see if the authors provide a reliable different perspective.

Their goal, openly stated at the very beginning, is to destroy the image of a literate Indus valley, to dismiss the idea that this civilization was the largest literate society in the early ancient world, and consequently to question the importance of the Indus script '[...] not only for ancient Indian history, but for human history as a whole' (p. 20) ([4]). The largest literate society in the early ancient world? Writing in the Indus centers was evidently restricted to a particular social group and to a set of specialized functions, and this certainly does not imply a large literate society. In Shang China (2nd millennium B.C.), writing was esoteric knowledge belonging to small groups of professional scribes and religious specialists, whose function was to ask the super-natural spheres for prophecies and suggestions about the king's and state's decisions. While oracle bones played an important role in Shang's politics, and have indirectly revealed a huge amount of historical information, there is no evidence that literacy played any function in administrating the state's economy or purposefully recording history, political propaganda and the like. Shang society, besides the narrow context of oracle bones rituals in a room of the king's palace, was a completely non-literate one. Although nobody might have seriously proposed that the use of writing in an early protohistorical society such as the Indus Civilization did make a 'large literate society', the authors start their paper by building an artificial image of the opponents' views, ascribing to them extensions that only fit the needs of their easiest and more opportunistic criticism.

In time, the progressive spread of writing to other functions and communication contexts, in Shang China and other cases of protohistoric writings, represent cases of slow but successful cultural exaptation. Sometimes exaptation – a rare contingency, indeed, in many forms of evolution – did not occur, and writing technologies died with their restricted contexts of use. I always thought that in prehistory, given the endemic scantiness of the

([4]) Subsequently (pp. 46-47) although admitting that '[...] the fact that ancient civilizations could not only exist, but flourish without writing has been known for decades', the authors state that their find will have a major impact on a still underdeveloped archaeological theory, that has continued to uncritically connect the use of writing to urbanism. Actually, as far as we presently know, the evolution of writing might have taken place casually in different civilizations, and might have quite casually developed for fulfilling different functions: in Mesopotamia, for accounting purposes in central institutions; in India, for controlling specialized craft and trade networks; in China, for oracular questioning. Equally casual might have been the apparent lack of similar inventions in the Kopet Dagh piedmont, where huge urban centres such as Namazga-Depe and Altyn-Depe in the 3rd millennium B.C. never evolved similar technologies, in spite of the stubborn attempts of the Soviet scholars to recognize a proto-writing code in the signs scratched on the female figurines. I am also convinced that the ziggurat of Altyn-Depe is a pure archaeological invention and a further effort of 'Mesopotamizing' the Bronze Age of Turkmenistan, but this is another story.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

archaeological record, similar events might have been much more common than we presently suspect. In the Lung-Shan (or Longshan) period of mainland China (3rd millennium B.C.), for example, there are cases of early undeciphered inscriptions, and the idea that they represent aborted writing systems should be carefully considered together with the alternative explanations about non-linguistic symbols expressing 'ritual iconography', 'mythological sign sequences' and similarly obscure historical entities.

Positional Regularities and Non-linguistic Symbolic Systems in the 3rd Millennium B.C.

Farmer, Sproat and Witzel then shortly review the long history of failures of the many attempts at decipherment – sometimes openly absurd – on the basis of Dravidian or Indo-Aryan hypotheses (pp. 20-22). They remark with irony the boldness of previous announcements of successful decipherment (but in future many, I fear, will remember the *hubris* and boldness of their own statements of victory!). It is well known that both the Russian and the Finnish attempts at decipherment were based upon the discovery of some statistical regularities in sign positions. The authors know they cannot deny this evidence, but dismiss these regularities as meaningless. On what grounds? Because '[...] statistical regularities in sign positions show up in nearly all symbol systems, not just those that encode speech [...]' (p. 20).

This statement, at first sight, might seem appropriate. For example, figuration systems on pottery in the 3rd millennium B.C. (completely ignored by Farmer and colleagues) share evident combinatory attitudes by the their inner components. In the painted decoration of Shahr-i Sokhta Buff Ware in Period II, about 2800-2500 B.C. (Biscione & Bulgarelli 1983), the graphic system is strictly rule-bound (Pracchia 1984), to the point that an attempt at structural analysis carried out in the past could explicitly exploit a para-linguistic decoding model (Gyselen & Lerouge 1983). Such 'rules', to a certain extent, may be reconstructed after the operational sequences of a process of partition of the vessels in fields and sub-fields, and filling such geometric sub-spaces with designs. In this light, the Buff Ware figuration system of Shahr-i Sokhta is generated by a hierarchical system of steps where a decision to a certain level affects the possible alternatives at the subsequent levels. Its structural logic, consequently, is syntactic rather than paratactic, as the detected positional regularities in Indus script might be (Salvatori & Vidale 1997) ([5]). While the overall variability in the final, individual designs is enormous (1591 different patterns on a total of 9479 painted potsherds), the Shahr-i Sokhta design system was based on a very restricted master list of basic motifs (see below for details and implications).

The rest of the important discussion concerning positional regularities in Indus script suddenly drops into the first part of long note 5. Here we dig out again that such regularities are similar to those seen in '[...] countless non-linguistic sign systems'. Among such countless examples, according to the authors, would range Near Eastern emblem systems of the 2nd-1st millennium B.C., discussed at length in the following pages of the paper (pp. 39-43) and

([5]) But the generation of complex signs by the means of ligatures might represent cases of syntactic transformations in the script itself. Ligatures are later discussed by the authors when dealing with 'singletons' or signs appearing in the corpus as isolated instances.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

modern highway and airport signs (at this point, one wonders why if these symbolic systems in prehistory are so common, and Farmer recorded and measured so many cases of contextual regularities, what need there is of referring to modern highway and airport signs, that – besides having admittedly very little to do with Indus archaeology – might be considered symbolic expressions as well as simple and repetitive logographic inscriptions, very easy to translate in rudimental linguistic correlates).

The other non-linguistic sign systems later quoted by the authors are Scottish heraldic blazons (at p. 27, see below), and the Chalcolithic Vinca inscriptions in south-eastern Europe (p. 39), for part of which, I believe, the non linguistic character is far less certain ([6]). No example from 3rd millennium B.C. South Asia is taken into account. One suspects that an accurate, cross-cultural quantitative search for positional regularities in non-linguistic sign systems contemporary to the Indus script was not performed by the authors, or, if they attempted it, the results were not as successful as implied (and this, as we shall see, for obvious reasons).

The basic problem lies with the nature of the examples they selected. I do believe that the Indus signs or symbols should be compared in first place with contemporary presumably non-linguistic graphic sign systems of the same general geographic and cultural areas, i.e., Central and South Asia, rather than with cases distant in space and time, such as the wild array made up of Scottish symbols, modern airport signs and Babylonian *kudurrus* of the 2nd-1st millennium B.C. proposed in the paper.

As a matter of fact, archaeologists painstakingly collected, in the last 25 years, some systematic archives of non-liguistic symbols coming from the Iranian Sistan and Kerman, Turkmenistan and the North-Western Province of Pakistan. I refer to the so-called 'potter's marks' notations encountered on ceramics respectively at Shahr-i Sokhta (2 different and independent systems of marks, one painted, dated around 2700-2600 B.C., the other incised, around 2400 B.C.: Biscione & Bulgarelli 1983; Tosi 1983: figs. 4-5), Tepe Yahya (3rd millennium B.C.: Potts 1981), Shahdad (about 2500-2000 B.C.: Hakemi 1976), Rehman Dheri (2800-2600 B.C.: Durrani 1981; Ali 1994-95) and Mehrgarh-Nausharo (*c.* 3000-2700 B.C.: Quivron 1980, 1997). Another important non-linguistic system of symbols is the painted figuration on the pottery of Shahr-i Sokhta, already mentioned above. It has been studied across a collection of about 20,000 potsherds kept at the Centro Scavi, IsIAO, Rome, about half of which are painted. To these systems one may also add the series of marks found by Soviet archaeologists incised on the female terracotta statuettes at the sites of Altyn-Depe,

([6]) I do not feel like entering in the Vinca problem. Looking at fig. 8, we have the three famous 'Tartaria tablets', 2 of which, due to the subdivision of the surface in separate fields and the association of possible pictographic signs and possible numerals closely resemble the early Uruk accounting tablets. Even the presence of suspension holes, indicating their possible attachment to goods or containers, supports the impression that these tablets might have belonged to an early script used in administration. For what the other 'inscriptions' are concerned, on the boar we doubtless have a decoration pattern; the female figurine has a vagina-like sign where it should be, and therefore the idea of an inscription is unlikely; and the object at the upper right shows a hardly intelligible loose sequence of strokes and crosses. I doubt that the three tablets have much in common with the other cases. Finally, contrary to what is stated by Farmers and co-workers, the geographical extent of the Indus script is hardly comparable to that covered by the Vinca signs, occurring in a relatively limited area corresponding to the basins of the rivers Tisza and Morava in the middle Danubian region.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

Namazga-Depe and other sites, interpreted as symbols of specific divinities and therefore possibly relevant to part of the authors' theses (from Masson & Sarianidi 1972).

The only record we have of the complex signs appearing on the red-ware of the rich graveyard of Shahdad is a preliminary publication by A. Hakemi (1976), the discoverer of the site, who thought to be dealing with a form of pictographic writing, and therefore actively widely searched the site for marked potsherds and vessels. His tables presented three different headings, namely *mots incisés* (348 signs), *mots stampés* or stamp seals impressions (262 cases) and *phrases pichtographiques* (250 sequences of 2 or more signs). The problem is that in general the signs are not copied with the necessary detail; the numbers provided in the text differ from those of the listed signs; his incised signs include many repetitions of identical signs and almost certainly some seal impressions. On the other hand, his seal impressed signs also include the famous proto-Elamite inscription translated by Hinz and sceptically mentioned also by the authors, and it seems that among *phrases pichtographiques* Hakemi listed also multiple seal impressions. Thus, this material is really tough to handle. Nonetheless, on the basis of the list and of the personal examination of part of this material at Shahdad in 1976-1977 I can say the signs incised on the red ware actually were numerous, highly variable and often quite complex, and that the multiple seal impressions on the same ceramics actually have no comparisons in contemporary contexts that I am aware of. The signs encountered on the pottery at Shahdad are definitely anomalous in the South Asian picture, and it is very unfortunate that they cannot be more properly analyzed. Preliminarily, I calculate the number of individual signs (excluding sealings) from Shahdad to about 100-150, but this is little more than a guess.

Next, I consider a sample of what, for 3rd millennium South Asia (besides the Indus), is one of the richest iconographic *corpora* of symbols presumably having mythological meaning and that, most probably, refer to complex narratives: the icons visible on the chlorite artefacts recovered from the ruinous illegal excavations of the graveyards along the Halil Rud near Jiroft (from *Dossiers d'Archeologie 2003*). The chronology of most of the Jiroft grave finds, on stylistic grounds, may be preliminarily included between 2600 and 2000 B.C., a good match with that of the Indus script. The Jiroft icons are also a relevant test, because they are rather close, in principle, to the complex iconographic symbols from the *kudurru* stones discussed at length by the authors (that are, at any rate, much later and much more distant from the Indus Valley). Note that the context of use and communication is completely different (for the *kudurru* stones, the symbols invoke the authority of a variegated ensemble of divinities for declaring, in front of the whole community and outsiders, the sacred nature of a boundary. The Jiroft stone vessels represented prestigious myths as ideological re-enforcement of elite groups during funerals). Furthermore, while in the case of the Mesopotamian religious emblems carved on *kudurrus* many symbols acted simultaneously in the same context, the Jiroft vessels and other ritual objects necessarily presented more limited symbolic associations that must have been perceived differently. I have selected from the Majidzadeh Catalog 52 well documented objects whose field of decoration might have included more symbols and have avoided objects carved in the shape of single symbols (like for example, the presumable gameboards in form of eagles/vultures or scorpion-men: see in *Dossiers d'Archeologie 2003* Majidzadeh Catalogue 130-33, 135-36, 141 and the like).

Finally, I have also included a preliminary survey of the symbols featuring on more than 400 seals from the early 2nd millennium B.C. excavated site of Faylaka (Dilmun) published

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

in Kjaerum 1983. I think this comparison is even more relevant both for the substantial amount of seals considered, and because the symbols appear in limited numbers on glazed steatite stamp seals whose manufacturing technology was in many ways similar to the Indus one (and, in general, for the close ties that connected Meluhha and the Gulf cultures at the turn of the 3rd millennium B.C.).

The ease with which the authors decided to ignore these readily available *corpora*, and consequently did not test on such sounder and more relevant grounds their interpretations, is almost admirable. The following table (Table 1) summarizes the basic features of 10 graphic non-linguistic systems from Central and South Asia of the 3rd millennium BC for which there is reasonable archaeological information.

At Mehrgarh-Nausharo, in 1265 cases of inscribed potsherds and vessels, were recorded 47 types of different marks and 38 examples of unique occurrences. In the Tepe Yahya master sign list Potts recognized about 20 sign groups (for example, lines, dots, combinations of lines and dots, V-like patterns, and so on) resulting into a total number of about 70-75 different signs (on a total of 353 potsherds and vessels). While the first cases date back to around 3000 B.C., the signs become more common in Period IVA (about 2000-1800 B.C.). About one third of the signs occur a single time. There are few complex sequences of signs apparently reminding poorly documented cases from Shahdad (Potts 1981). At Rehman Dheri, the excavators found a relatively complex corpus of signs inscribed before firing near the bases of the vessels. 314 sherds showed a system of 60-70 signs, rather similar (but more elaborated from a graphic viewpoint) to those from Shahr-i Sokhta. Out of this code, no more than 4-5 signs were patterns encountered also in ceramics. Painted signs are present (sometimes below the containers' rims) but rare. The excavators do not provide a clear distinction between the signs thus differently drafted. Incised signs were traced on the soft clay from right to left (Durrani 1981; Ali 1993-94). While Durrani (1981: fig. 4) may have over-emphasized the similarity of these signs to those of the Indus script, it seems that a good number might be considered indirect antecedents of later standardized characters of the script itself. At Shahr-i Sokhta, we have respectively a series of no more than 35-40 basic designs (e.g., semi-circles, triangles, festoons, zig-zag lines, stepped lines, stags, fish, etc.) in painted figuration, 35 painted and 36 incised types of marks.

The number of individual signs used on southern Turkmenian female figurines datable to Namazga V (perhaps used in rituals for dedications to different deities) might range from a minimum of 6 basic signs to a maximum of 26 variations (Masson & Sarianidi 1972: 135); and a total of 27 symbols or icons on the chlorite artefacts of Jiroft is combined in very different scenes containing up to 4-6 actors and might suggest quite different mythological-religious environments. Finally, the apparent endless diversity of Dilmunite steatite stamp seals is generated by no more than about 45 different individual symbols, appearing, to use the authors' words 'a complex mixture of abstract symbols and iconography' whose basic meanings probably were 'complex associations that linked deity signs to celestial, terrestrial and social phenomena' (pp. 39-40).

With the still doubtful but relevant exception of the marks on the red ware at Shahdad, in each context the symbolic systems considered used a rather limited number of signs (from about 25 to a maximum of 60-70). If one can generalize on the base of such a composite ensemble, a non-linguistic symbolic system of South Asia in the 3rd millennium B.C. included on average about 42-44 different signs (again excluding, for the moment, Shahdad). Such a value, probably, is too high, because the recorded marks in many cases belonged to different periods and were not, strictly speaking, contemporarily in use.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

Table 1. Basic features of 10 graphic non-luinguistic systems of symbols from Central and South Asia of the 3rd-2nd millennium B.C.

Sites	Years B.C.	Context	Technique	Signs no.	Regularities and associations, basic features
Mehrgarh-Nausharo	3000-2700	Pottery	Incised (marks)	45	Isolated marks; repetition of elementary traits to form individual signs; rare associations of 2 different signs in the same vessel
Tepe Yahya	3000-1800	Pottery	Incised (marks)	70-75	Isolated marks; repetition of elementary traits to form individual signs; rare associations of 2 or more different signs in the same vessel
Rehman-Dheri	2800-2600	Pottery	Incised (marks)	60-70	Mostly isolated marks; aggregations of different signs such lines, nail impressions, V-shapes, crosses. Few cases of 3 or more signs repeated. 4-5 signs relate to pottery figuration; a dozen related to Indus signs. Rare cases of compounded signs similar to Indus cases
Shahr-i Sokhta	2800-2500	Pottery	Painted (designs)	35-40	Isolated designs, repetition of elementary traits to form redundant designs; same designs repeated to form continuous fields; use of complex, rule-bound syntatics, generating from few elements an almost endless variability in finished patterns; unique designs are rare
Shahr-i Sokhta	2700-2500	Pottery	Painted (marks)	35	Isolated marks, repetition of elementary traits to form individual signs, signs coupled in very rare instances
Shahr-i Sokhta	2500-2400	Pottery	Incised (marks)	36	Isolated marks, repetition of elementary traits to form individual signs, signs coupled in very rare instances
Shahdad	2500-2000	pottery	Incised (marks)	100-150?	Isolated marks, rarely coupled; aggregations of different signs in unique complex forms; some sequences combining stars, lines, dots and other basic signs
Altyn-Depe and others	2500-2000	Terracotta figurines	Incised (marks)	26	Isolated marks; repetitions of identical signs in the same figurine; symmetric coupling of 2 different signs in a single image
Halil Rud (Jiroft)	2500-2000	Chlorite objects	Complex carved icones or symbols	27	Same icones repeated to form continuous fields, frequent coupling and opposing of recurrent icones, few narrative sequences with 4-6 actors; unique symbols or narrative sequences are present but rare and quite distinctive
Faylaka (Dilmun)	2000-1800	Steatite seals	Incised icones or symbols	45	Never isolated; repetetive associations between 2-4 signs or more, following axial and rotational symmetries; high redundancy, by symmetric repetition of the same symbols and secondary additions; unique signs are present but very rare

I deem these cases, for chronological, archaeological and historical reasons relevant comparisons with the Indus script, and I deny that contemporary airport tags and Scottish heraldic blazons have something to do with the questions under scrutiny.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

This figure is interesting, because it regards quite different symbolic systems, including potters' marks, designs on painted vessels, iconography on presumably ritual objects and seals. Potters' marks, most probably, were mainly used for various organizational tasks in production. The communication target of painted decoration (more or less unconsciously) was the much larger context of users of painted vessels. Although these precise numbers might somehow vary according to the criteria used in classifying and distinguishing the signs themselves, it is anyhow obvious that we are very distant from the 400 signs and more identified in the Indus script, a corpus approximately 10 times more capable. In other words, the high number of signs alone sharply distinguishes the Indus inscriptions from any contemporary known graphic or iconographic symbolic system of South and Central Asia. On the same side falls the Proto-Elamite writing encountered in the late 4th millennium B.C. at Tepe Yahya, where on a small group of 27 administrative tablets appear not less than 80 different signs (including numerals) (Damerow & Englund 1989: 65-76). As Farmer, Sproat, and Witzel like this kind of argument, I challenge them to identify another South Asian system datable to the 3rd millennium B.C. of non-linguistic signs amounting to 400 basic signs or more.

It is clear that the inclusion of such restricted (but in their contexts presumably efficient) symbolic systems in their samples would have highlighted the non-comparability of the Indus script to such codes, thus lessening the impact of a good part of the authors' arguments. This is why, I believe, these systems were not considered. It is also clear that in the known contemporary systems, non-linguistic symbols behaved quite variably, and that archaeological data lead us to question the superficial claim that positional regularities are easily found in 'countless non-linguistic sign systems'.

The reality is much more complex and intriguing. In potters' markings, signs occur mostly in isolation, rarely in couples, and even more rarely do such couples appear in repetitive associations (again, with the possible noticeable but still mysterious exception of Shahdad). In these cases, positional regularities are largely ruled out. In contrast, painted figuration on ceramics might have involved large-scale and strict positional rules possibly more consistent and visible than those observed in early scripts (but depending upon syntactic). Turkmenian figurines sometimes present simply coupled signs and the same sign is often redundantly repeated in different parts of the figurines' body. Together with coupling and opposition of selected symbols, systematic, large-scale redundancy (constant repetition of the same designs or symbols) is a distinctive feature shared by the more evolved and formally elaborated non-linguistic symbolic systems considered (highly repetitive patterns on the pottery of Shahr-i Sokhta ([7]), 'endless' repetition of icons such as scorpions, men-scorpions, temple facades, water-like patterns and interwoven snakes at Jiroft, and redundant specular doubling of most major symbols in the Dilmunite seals). While positional regularities might be detected in part of the Jiroft figuration, redundancy in all these systems dismiss one of the basic assumption of Farmer & others, who take the rarity of repeating signs as a proof of the non-linguistic character of the Indus script. Archaeological evidence demonstrates that non-linguistic symbolic systems are often redundant. Besides the common

([7]) For a preliminary study of the figuration system on the Shahr-i Sokhta Buff Ware of Periods II and III, and its changes through time (including a quantitative assessment of diachronic variation of its relative information potential by the means of Shannon statistic tests), see Vidale 1995.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

symmetric duplication, for example, the Dilmun seals show the very frequent insertion as secondary filling units of symbols such as fowls, circles or grids, which intensify the pattern and somehow provide a unifying network spreading across great part of the seals' iconography.

Moreover, unique signs, designs, icons or scenes are rather rare in part of the systems considered in table 1 (for example, in the figuration system on the pottery from Shahr-i Sokhta or in the icons on the chlorite vessels from Jiroft), while they are very common in the potter's marks from Tepe Yahya, Mehrgarh-Nausharo and possibly Shahdad. This, too, does not support their assumption that unique signs in the Indus script should be mechanically interpreted as a demonstration of its non-phonetic nature. These are just preliminary remarks, but it is evident that we may probably learn more from these systems than from cursory looks to Scottish heraldry. In some graphic systems, such as the potters' marks at Shahr-i Sokhta, Mehrgarh-Nausharo and Rehman Dheri, graphic complexity appears inversely proportional to frequency, while Indus signs occurring only once may be quite simple (see for example the 2 unique signs on the lapis-lazuli cylinder seal of the Schøyen Collection – besides being relatively simple in graphic terms, they are also elementary modifications of other recurrent Indus signs).

Finally, Farmer and his colleagues remark that '[...] third-millennium scripts typically omitted so much phonetic, grammatical, and semantic data, and used the same signs in so many varied (or 'polyvalent') ways, that even that we are certain that a body of signs encoded speech, it is impossible to identify the underlying language solely from such positional data' (p. 20). Also this sounds acceptable: positional regularities in sign sequences alone do no demonstrate by themselves the linguistic nature of the system (even if a similar hint may hardly be considered secondary). On the other hand, for the Indus script such regularities, continue the authors, have been 'grossly exaggerated' and '[...] can only be maintained by ignoring or rationalizing countless exceptions to the claimed rules' (p. 21). But what about the flexibility and the polyvalence of the signs' use they just called in play few lines above? These features alone might possibly explain the evidence of variability and exceptions to the prevalent norms. The authors contradict themselves when they advocate flexibility in reading for conveniently denying the possible linguistic implications of positional trends, but immediately after state that the many deviations and exceptions lower the value of the detected regularities. Circular argument.

Directionality in the Inscriptions

That Indus inscriptions were normally read from right to left and, in the case of longer texts with superimposed lines, from left to right, in a bustrophedic fashion, cannot be denied even by the authors. For this reason, I believe, directionality follows positional regularities and fall, too, in the capable container of note 5, p. 21, thus preventing them from further embarrassment. Obviously, directionality alone does not make a script (see for example the *kudurrus* at p. 41, where the symbols with zoomorphic features consistently look right), but together with positional regularities, it shows that the Indus script was a structured and standardized system (and this does exactly not fit with their interpretations). As Farmer and others are expert users of scientific shoehorns (as Steven Jay Gould would have said), directionality is compressed in little more than three lines of the same note, where we have

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

even the time to read that, at any rate, they have brilliantly envisaged in some inscriptions mythological narratives that can be read sequentially (for this pretended 'discovery' see Priyanka 2003 and comments below).

Without much confidence, they try here do dismiss directionality by defining it solely in terms of carving: 'The fact that Indus inscriptions tended to incised in one directions or another, which is a predictable output of workshop habits, does not mean that they were meant to be 'read' in a fixed direction [...]'. Certainly, if you beforehand believe that the signs are not writing. Actually, this closely depend upon an important unanswered question: did the carvers master and understand the signs, or did they copy in the workshops inscriptions traced by professional scribes on steatite or other media? Unfortunately, we do not know. For the moment being, anyhow, directionality in writing reasonably hints to directionality in reading. For what 'workshop habits' are concerned, even a preliminary survey of unfinished seals published in the *corpora* edited by Parpola and co-workers (where the pictures are anyhow insufficient for a detailed technical analysis) shows the manufacturing sequence of steatite seals, at least in many instances, was far from standardized. Unicorns, 'standards' and inscriptions were often carved in free sequential order with variable techniques and even some stages in the cutting process of the seal itself might have been inverted (Vidale, ongoing research). Thus, as evidence rules out 'workshop habits' as an inner feature of Indus seal production, directionality cannot be simply explained as proposed by Farmer and others. While questioning one of the oldest and worn commonplaces of craft production studies in protohistoric societies, the lack of standardization in the seals making process has some important implications for the following discussion.

Dominance of High Frequency Signs

> All major studies agree that a small number of symbols dominate in Indus inscriptions. Just four of 417 signs account for 21% of the 13,372 sign occurrences in Mahadevan's concordance; eight signs make up 31%; and twenty signs over 50% [...] Our statistical studies of different classes of Indus inscriptions [...] confirm that the dominance of high frequency signs is typical of all inscription types, and is not an artifact of the artificial conflation of inscriptions of different classes in the existing concordance and catalogues. (pp. 26-27).

Unfortunately, these statistics are not presented in greater detail in the paper, in spite of its considerable volume. As the Indus corpus is largely dominated by inscriptions on seals and on tablets (sometimes moulded from seals and seal-like moulds) a comparison between these classes and a completely unrelated class such as inscriptions on pottery would have been quite revealing. I will return later to this point.

The authors present a graph (their fig. 2) where on the x-axis individual Indus signs in Mahadevan's concordance and a catalogue independently drafted by B. Wells are separately plotted in ascending order of frequency (from 0 to 600 and more), while on the y-axis we see the relative cumulative percentage contribution. The Indus samples are accompanied in the graph by '[...] six Sumerian texts [...] four hieroglyphic texts [...] a selection of Chinese newspaper stories [...] a selection of newspaper headlines from the same source [...]' (n. 12).

Perhaps these texts were selected for this comparison or test on the basis of standard statistical or computational considerations, but for an archaeologist not accustomed to

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

computer engineering in linguistics is really hard to understand how few Sumerian and Egyptian texts might provide a valid quantitative reference: for Mesopotamia, one would expect that a lexical tablet, an economic text, a royal genealogy and a tablet reporting a piece of epics would have quite different sign frequencies, and that the chronology of the texts, too, would seriously affect the counts. Here, probably, Sproat gives for granted a know-how that I personally miss (even the attached n. 14 is soaked with technical jargon quite difficult to follow for most readers).

At any rate, the Indus signs, Egyptian hieroglyphs and Sumerian cuneiform writing nearly overlap in the same skewed curve, i.e. share the same dominance of few high frequency signs combined with many low-frequency ones (this is why many scholars have reasonably considered the Indus script a syllabic system, provided, because of the high number of signs, of logograms, determinatives and other special function signs: see at p. 29). Chinese headlines and Chinese news texts have a much smoother trend ([8]). The authors, at this point, insert in the graph '[...] a large body of heraldic signs [...] encoded in the "blazon" system used since the middle ages to analyze coats-of-arms' (p. 27), for some reason from Scotland. May be one or more of the authors belong to aristocratic Scottish families and daily handle such staff, but the surprised South-Asian archaeologist, at this point, has not the faintest idea of how this highly prestigious system of formal analysis works. I guess, for example, that heraldic signs, being internally subdivided and including composite symbols, might be analyzed (like the designs on the pottery of Shahr-i Sokhta) at different levels of aggregation with quite different results, and would have been interesting to know more about the Author's choices. At any rate, the curve of these blazons coincides with Egyptian, Indus and Sumerian script systems. As it now turns out that studies of general sign frequencies cannot reliably distinguish scripts from non scripts and therefore are useless, one wonders why she or he had to go through the graph and bear the considerable burden of note 14, when might have well directly landed to p. 29, and finally, as the authors promise, '[...] approach the sign-frequency from a different angle'.

Low Sign-repetition Rates in Indus Inscriptions

So, we start again with the statement that 'High sign frequencies are normally fairly reliable markers of high levels of sound encoding in the scripts, reflecting sound repetition at some level in the underlying languages'. (On the other hand, as we have seen, high sign frequency and redundancy may also distinguish purely symbolic, non-linguistic communication codes). According to Farmer & co-workers (p. 29), anyhow, 10 of the 20 highest frequency signs in Indus inscriptions do not repeat themselves, or repeat very little, across the whole corpus of Mahadevan (2905 inscriptions). As we have been previously informed that the 20 commonest signs account for around 50% of Mahadevan's signs occurrences – although statistics here are badly formulated – we may assume that a

([8]) A non-specialist reader might comment that these curves might simply reflect the gradually wider semantic context of the analyzed inscriptions. Also, when Sproat states that such studies can show that the Indus system could not have been a Chinese-style script (p. 29), the same reader might ask if he really needed a powerful computer to come to this conclusion.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

consistent part of the texts (perhaps 25%?) shows repetitions of some of the commonest signs (it would be important to know, at this point, which signs exactly repeat themselves, and which ones do not, and if the relative order of frequency is linearly correlated with the repetition in single texts, or not; referring only to the 20 commonest signs as a whole does not help that much).

The authors than admit that 'In many ancient scripts, some high-frequency signs can be expected to show up only once in certain types of short inscriptions: in seals inscriptions or name lists, this is true of gender determinatives and whole-words signs standing for titles or divine names incorporated in human names' (p. 30). If the counts of Farmer and others are correct, I think, like many others, that this is possibly what might happen in a good part of the Indus seals texts. But the authors, without further proof, simply assume and give for granted that the rarity of repetitions the envisaged '[...] suggests that those inscriptions contained little if any phonetic coding' and this, they argue, is true also for signs modified by ligatures or created by compounding different signs (this is quite understandable, because we know that such signs are very rare and sometimes unique occurrences).

They go on (p. 31) declaring that the combination of high general sign frequency (common to the other systems of the 3rd millennium B.C.) and low repetition rates in single inscriptions does not pertain to 'fully enabled scripts' (enabled for what?), forgetting, this time, the possibilities mentioned few lines above. To support this new extension, they show what they call a 'short inscription', actually a fragment of a Luwian monumental hieroglyphic inscription from Anatolia (early 1st millennium B.C., see their fig. 4), where some signs are consistently repeated. Now, in first place the inscription is not short, including about 40 preserved signs in two registers, more than the double of the longest known Indus inscription. Secondly, it is a minor part of a much longer text on the nature of which we are not directly informed. How can a monumental, presumably royal inscription with a quite complex content and formal or ritual encoded information be compared with the short Indus texts, probably dealing in a completely different way with a quite different content? I might be wrong, but to me, it is like comparing a random sample from the Manhattan telephone directory with a group of early Christian grave inscription in Greek, and saying that the absence of long sequences of numbers in the second group demonstrates that early Christians could not count. Similar results might be interesting in terms of computing experimentations, but hardly (I feel) for archaeology and history.

Egyptian Cartouches

The authors might have realized the scarce impact of such a 'demonstration', because they quickly move to another comparison, this time with ancient Egypt (pp. 31-32). They casually select 67 cartouche inscriptions with names of pharaohs including 6 signs or more (without providing any chronological information). Their average number of signs per cartouche is 6.94. Then they extract from the *corpora* of Mohenjo-Daro and Harappa a comparable group of inscriptions of 6 signs or longer, count their signs and repetitions and show the results in their table 1. As sign repetitions in the Egyptian cartouches are about 6-7 times more frequent than the Indus cases, they state that their 'dramatic' results show that Egyptian inscriptions are phonetic, the Indus ones not. I independently checked these conclusions, using a random selection of royal cartouches published in Baines & Malek (1985: 37), ranging from Narmer to

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

the Roman emperor Septimius Severus. Taking into account 74 cartouches, I got a total number of 506 signs, with an average number of signs for inscription of 6.83 (a good match with the authors' figure), and 57 cases of repetitions of the same signs. The average of sign repetition per inscription in the authors' sample is 0.71, in mine 0.77: slightly higher, but another good fit. This indicates that the authors' sample was not basically different from mine. But when I subtracted from my inscriptions the long and complex cartouches of the pharaohs of the Greek-Roman period (that, incidentally, bore also Greek and Latin names), the average number of signs per cartouche lowers to 5,13 and the average repetitions per inscription 'dramatically' drops to 0.40, much less than the authors' published estimate. Although there is little doubt that in Indus inscriptions sign repetitions are much less frequent than in the Egyptian royal names, according to their own calculations the rate, at least before the beginning of the 3rd century B.C., would be $^1/_4$ and not $^1/_6$-$^1/_7$ as they state at p. 33.

But there are other interesting points. Egyptian cartouche inscriptions are complex royal epithets and statements with developed grammatical structure and distinguished by an absolute dominance of high frequency signs, usually indicating divinities (for example, like in the names Men-kau-ra or Tuth-ankh-amon). Names in cartouches, in the Middle Kingdom, typically came in couples, the first one being a throne name and the second a birth name. Divinity signs usually dominate throne names and are less common in birth names (the ones we presently use to identify the kings). So, also in this case, the selection of one type of name and cartouche or the other is not neutral (in the sample I used, from Middle Kingdom onward, both types were systematically represented). The authors moreover admit that phoneticism – i.e. evidence of repetition of signs with phonetic value in the same inscriptions – is more developed in their longer cartouches, that certainly are not those of the 3rd millennium B.C. This check shows that if while selecting similar samples one does not provide the needed archaeological and chronological details – that means, if contexts are not accounted for and carefully evaluated – there is a wide margin of uncontrolled interpretation and perhaps involuntary bias.

Incidentally, one also notes that in some inscriptions, when possible, repeated signs were arranged in symmetric patterns which the authors might have interpreted as oddities being 'decorative or symbolic in nature' (p. 35) (see for example the inscriptions of Pepi II or Sesostri III). This shows that such forms of symmetry in signs position cannot certainly be taken as indicators of a purely symbolic nature of the Indus script, as the authors insistently propose.

On the whole, I think that such a poorly constructed Egyptian comparison, far from demonstrating that Indus texts are not phonetic, only suggest the obvious, that Indus inscriptions cannot be easily compared with the cartouches (after all, nobody ever expected to find on the Indus seals names of kings), as they probably encoded different information and might well follow different writing and reading approaches, presently impossible to reconstruct. Chronology, also, is a key variable in the Egyptian names as well as in the Indus texts.

Unique and Low Frequency Signs

The authors underline that a large number of signs of the Indus script appear in single instances (27%), or with very low frequency (supposedly more than 50%). They support these statements on counts from Mahadevan's concordance (1977), on the credibility of which, anyhow, they cast doubts. These counts evidently depend upon the way the signs are

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

defined, grouped or finally classified, and the uncertainty, here, given the general disagreement among scholars on the precise number of individual Indus signs, is maximum. For example, Farmer & colleagues quote Wells with 600 signs out of 7165 sign occurrences, with 50% of the signs appearing only once, and 75% appearing 5 times or less. But in note 23 they report that to the same Author has later gone from one extreme view to the other, and presently thinks that almost all the unique occurrences regard complex signs, i.e. signs obtained by combining other signs by the means of graphic incorporation. Similar statistics, for the moment being, are evidently useless. The authors think, anyhow, that the 'majority' of single occurrences are not compounded or made by the means of ligatures. As they seem to assume Mahadevan as their basic and only reference, we are left with a rate of 27% of single signs minus a minority of signs obtained by graphic incorporation. Consequently, even if they do not report precise counts, we would be dealing with an estimate of about 15-20% unique signs. On the other hand, Farmer and others have to admit that unique signs, after recent studies, are well represented in early cuneiform and proto-Elamite systems as well (p. 37). They provide *per se* no demonstration, therefore, of the hypothesized loosely symbolic nature of Indus script. Even the authors are well aware of the inner weakness of this argument, if in note 23 confess that the find of rare signs is quite secondary and not central to the overall proofs they think to have otherwise presented in their paper.

At any rate, they also state (somehow paradoxically) that the evidence of unique signs in proto-cuneiform and proto-Elamite supports their conclusions, because these systems would be 'largely decoupled from spoken language', as they think the Indus script was (p. 37). Now, numerals and pictographs in early accounting systems, belonging to lists and counts of goods and people, might be viewed as 'decoupled' (whatever this means) or not from language (this is probably a problem of ours, not of the ancient living system), but these cases have nothing to do with the Indus script. Indus inscriptions on seals, ceramic and the rest cannot obviously be considered, as a whole, the same type of simple administrative texts. The authors, once more, are unwilling or incapable to consider the functional contexts they discuss.

Without providing any objective supporting evidence, they moreover state that as unique and rare signs '[...] keep cropping up with each new batch of discoveries' (p. 36), the Indus cannot be a true script. Unique and rare, in this context, are not the same thing. While the find of more specimens of formerly rare signs would only lessen their arguments, it would be the continuous discovery of new signs (if this is true, but for the moment we have to rely on the authors' preliminary impressions) to require further comment. Their explanation is that the signs are loose system of symbols, somehow light-heartedly invented by everybody to be soon and carelessly abandoned.

I think that unique or rare signs in the Indus script might have other possible explanations. In the Mesopotamian world and, in general, in the ancient Near East writing was managed by central institutions such as royal palaces, by the means of organized schools for scribes. This is punctually reflected in the archaeological record. In the Indus cities, there is no evidence of such schools; we have only dumping layers with steatite debitage from seals making and some unfinished seals found in unclear connections with buildings or building foundations fillings ([9]). There is not a single steatite seals making workshop clearly identified

([9]) For the organization of steatite production at Mohenjo-daro and in general in the Indus cities, see Vidale 1987a, 1987b, 1989a, 1989b, 1990, 2000. For seal making, see Rissmann 1989 and Franke 1991.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

in a central urban location, such as a major building in a Citadel. In contrast, seals making and writing on seals seems to appear in different, spatially scattered spots. Apprenticeship, in these contexts, might be revealed by errors and uncertainties in seals making process. Not many years ago, a deposit with dumped refuse from a seal-making unit was found in the sands of the present Indus bed, almost 2 km south of the Citadel (Sharif 1990).

The models of urban growth emerging from the excavations at Harappa, and current re-consideration of some key architectural features excavated at Mohenjo-Daro (Vidale, forthcoming paper) pointed out that the traditional univocal dichotomy Citadel-Lower Town should be abandoned. In contrast, the different walled mounds or compounds growing and taking form at Harappa might be considered, at last in some moments, as partially independent seats competing for power with alternate fortunes, in a hierarchic political framework (i.e. potential 'Citadels' in their own). This idea might account for the presence of different workshops or craft families independently involved in seals making, the lack of standardization in the seals manufacturing sequences, and a certain degree of variation and independent invention by the scribes. While in the west (after the phases of political unification of Mesopotamia and particularly in the course of the 2nd millennium B.C.) the need of storing, retrieving and exchanging information on wider and wider contexts promoted a large scale standardization of cuneiform systems, in the Indus cities writing might have been mainly bound to the control of trade and craft activities within different urban compounds and the social groups to whom they belonged, therefore developing, although in the frame of a unified tradition, sets of individual signs. The relatively high number of unique signs and symbols encountered in the seals at the possible Indus enclaves in the Mesopotamia, in the Gulf, in the Iranian plateau and in Bactria (Parpola 1994; Vidale 2005) actually supports the idea that Indus scribes created new signs and continue to adapt modified forms of their script to their changing needs and to new forms of linguistic exchange. Similar hypotheses, obviously enough, will be useful only if archaeologically testable in the future on the field or on the data. For the moment, they are conjectures like many others (but legitimate and fully compatible with the traditional hypothesis of writing).

Near Eastern Emblems

At the end of their unmethodical, happy-go-lucky rides across space and time, Farmer & co-workers finally reach the 'Near East'. Here they start speaking about '[...] symbol or emblem systems whose development can be traced at minimum from the fourth millennium to the Hellenistic era [...] on seals, stelae, plaques, boundary stones (*kudurrus*), cliff walls, friezes, amulets and many other media from Egypt to Eastern Iran [...]' (p. 39). Please note that this nonsensical definition unifies and equates to their 'emblem symbols' the entire field of archaeology and art history of Egypt, the Near East, Anatolia and South Asia, from pre-dynastic Egyptian ivory tags to Ubaid ceramics, Hittite rock sanctuaries, Achemenian reliefs and, I suppose, Parthian art. From this admittedly capable bag ([10]), at any rate, they select a neo-Assyrian relief and 2 Babylonian boundary stones with series of religious symbols, and crown

([10]) Only in texts inspired by new age fundamentalism one sees this kind of historic-cultural hotchpotch.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

them, at p. 41, with 41 emblems. Here these symbols are neatly drafted like computer fonts and deprived of the temple-like basement on which (on the boundary stones) they regularly figure.

We are informed that these deity signs, part of a much larger number (but how many, actually, are they?) have much in common with Indus signs, and somehow show, in their opinion, that the Indus signs, too, refer to divinities and their cosmological bonds with stars, star clusters and planets (while I write this, I have a very remote feeling I might have somewhere read similar ideas in some previous attempted decipherments, but I am certainly wrong). My first-sight impression is that the Mesopotamian symbols are very different from Indus signs. In first place, they are consistently associated to the depictions of temple-like basements, clearly stating their divine nature. Secondly, they are obviously representational, and in about half of the cases they represent animals or supernatural beings with animal parts; in fact, abstract signs without clear organic or formal referents are few, while in the Indus script purely abstract signs are abundant. Third, their high visibility on public inscribed stones matched very well with their intended duration, while Indus inscriptions on seals, visible only to a closer inspection and at a closer distance – by individuals, and not by crowds – were ephemeral impressions on unbaked clay tags made to be rapidly destroyed and immediately replaced. While wondering why a proposed parallel should be seriously taken as a demonstration of historical coincidence or functional identity, we may abandon without deep regret the reported wide distribution and very long history of the authors' 'emblem systems' and consider the actual context of the symbols they actually discuss.

Such emblems, in the Assyrian-Babylonian culture, and in general iconography, did not play a major role in Mesopotamian religion. They are non-narrative expressions concerning cults and related aspects of the human world, officially sacralized through processes that may lay beyond our capability of reconstruction and understanding (Oppenheim 1980: 157). In Babylonia and Assyria, the production and use of this class of images depended upon particular templates and historical processes, involving the traditional politics of manipulating divinities as protectors of individual cities, families and kings; the idea that enemies and opponents, in contrast, were unfit as 'godless'; 2 millennia of superimposition and mingling of quite different religious strata; the influence of a powerful and composite clergy committed to an endlessly growing ritualism; a tradition of praying, spells and magic largely based on the direct invoking of individual deities for specific purposes. Applied to boundary stones and official monuments, together with royal portraits and long inscriptions, these easily recognizable symbols made sure that even illiterate onlookers and strangers might have immediately perceived that the king's power and his actions were sanctified by the will and the support of a powerful pantheon. Such symbols spread in the first centuries of the 1st millennium BC, in times of wide-scale conflicts and great political uncertainty, to reach a peak in the neo-Assyrian and neo-Babylonian period, before the Persian conquest. In this last time, these icons are described as inspired to a purposeful archaism as well as to

> [...] symbolism (in figuration) and ritualism (in behaviour). Culture, now incapable of generating and controlling new values, pays a primary attention to formalities. Formalism is outstanding in religion: performance or not of rituals, the ways divinities, their statues and symbols were presented, the spelling of their epithets and of the right formulas, become the inner substance of private and public religion, and a testing bench for the king's trustworthiness. The use of symbols grows to the detriment of anthropomorphic images of the deities, and the repetition of traditional litanies substitutes the creation of new ceremonial or mythological plots. (Liverani 2004: 648, my English translation).

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

Would all this, or part, apply as well to the Indus valley? We are not told. For the authors, contexts are meaningless. But in the meantime, they have piled up new and more demanding conjectures on their own original supposition. The difficulties in understanding some Assyrian-Babylonian signs would reflect an 'inherent plasticity in the meaning' (p. 40: to me, it reflects in first place 'plasticity', in more mundane words uncertainty, in interpretation) and this, too, is true for the meanings of Indus signs; as the meaning of Mesopotamian symbols, in time, changed with the replacement or merging of major deities, this necessarily happened for the Indus signs as well; and conversely, as in Mesopotamia more signs stand for the same divinity, even many different Indus signs can point generically to the same god or goddess (pp. 40-42: this point is discussed at length strategic for the authors, who otherwise could not easily explain the large number of signs in the Indus script). Indus signs with evidence of compounding would equally indicate a similar merging of different divinities (p. 42). The readers may figure out on their own how far this mess of groundless conjectures provides a reliable, factual demonstration.

Some Possible Implications on the Historical View of the Indus Civilization

Although the authors make clear beyond any doubt that reading the Indus signs is practically worthless (p. 43), one line below we learn that '[...] evidence that Indus inscriptions did not encode speech increases, and does not decrease the symbols' historical value'. If you feel like breathing in relief, you can. The fact, continues the text, is that by the means of the described fluctuations of such vaguely semantic symbols, we may access '[...] measures of religious, political, and economic developments that are otherwise impervious to historical analysis, even if the meaning of most signs remain uncertain'. This positive belief in the power of True Science of handling prehistoric symbols, that might have delighted the young Hodder of about 20 years ago, after pages on pages of sharp criticism and denigratory irony on the others' views, is almost touching.

Perhaps for the authors the historical value of Mayan stelae has decreased, and not increased, now that the glyphs are not anymore considered symbols of '[...] families, clan, offices, cities, festivals, or professions [...] with specific gods of their celestial correspondents' (pp. 42-43, I am adapting the review to the Mayas) but as historical documents talking about kingdoms, enthronements, wars, captures, sacrifices. After all, now the Mayas have lost something of their supposed hieratic, peaceful halo, so dear to the American establishment controlling Mesoamerican archaeology till the very end – when they had to surrender at the evidence generated by computers in an obscure Soviet laboratory. Nowadays Mayan elites appear now stuck with problems depressingly similar to those faced by other early state lords in Sumer, in the Huang-Ho valley and in sub-Saharan western Africa in different periods of human history.

But the Indus Civilization, with its elegant, mute signs, provides a comfortable cylinder hat from which one can easily pick up the rabbit of other esoteric characters, doubtless charming, again, to a large audience. See at p. 44, where 'oddly shamanic looking elites' suddenly pop off. The detection of shamanism through iconography (frequently applied, for example, in rock-art analysis) is notoriously full of pitfalls, and the assumption that the elites included shamans is completely arbitrary. That the images in the miniature tablets and in the seals do represent members of the Indus elites is another unsupported pure guess (as far as we know they might as well be djinns, demons, heroes, priests or divinities or something else). Nevertheless, with a highly creative jump that leaves little to envy to some of the most imaginative or politically-

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

biased proposed decipherments, we are seriously informed that these were the elites that by the means of a 'simple' system of 400 symbols ([11]) controlled Indus society as a whole. And there is more. While the use of these non-linguistic signs explains the wide extent of Indus culture and its cultural uniformity in a polylinguistic world, Indus elites might have politically opposed writing because it would have threatened their power.

A Little Forgotten Magic Word

The little, magic word that Framer, Sproat & Witzel carefully avoided to mention is 'administration'. In the fascinating, cartoon-like picture they try to build and to glue onto the Indus Civilization – a narrative of powerful shamans, uttered myths, dark rituals (p. 36, n. 32), transmogrifying deities and evocative twinkling symbols – there is no place for such cheap materialist activities. Actually, archaeological data show that Indus script was largely used as an important component of the information conveyed by steatite stamp seals, and that these seals were frequently impressed on special lumps of fine clay applied on doors, lids of vessels and chests, packages. At Mohenjo-Daro stamp-sealed tags were found around the mouth of special vessels coated with straw and chaff, used to fire at high temperature in large vertical kilns stoneware bangles (Halim & Vidale 1984; Vidale 1993). This is one of the few direct proofs of the use of a seal for managing craft production so far identified in the whole archaeological territory of the ancient Near East and South Asia. Table 2 gives a preliminary list of the Indus sites where, as far as I am aware, sealings with steatite seals impression and writing were found, together with the reported or inferred function of the sealings (references, being excavations reports in the range 1931-2004, are presently omitted for sake of brevity).

Table 2. Reported occurrence and functions of sealings with steatite stamp seals impressions (mostly with unicorn and the inscriptions above) from the Integration Era of the Indus Tradition (*c.* 2600-1900 B.C.). Both at Nagwada and Bagasra in Gujarat there are various specimens of sealings, but the rears are not fully published and the function can be inferred only from the reports.

Site	Jars closures	Doors, partitions	Wooden boxes	Packages with reeds, cloth, string	Stoneware bangles firing vessels
Mohenjo-Daro	X	X	X	X	X
Harappa	X	—	?	X	—
Chanhu-Daro	?	—	—	X (?)	—
Nausharo	—	—	—	X	—
Kalibangan	X	—	?	X	—
Lothal	X?	X	X	X	—
Rakhigarhi	—	X	—	—	—
Banawali	X	—	—	—	—
Nagwada	—	—	—	?	—
Bagasra	—	X	X?	?	—

([11]) How and under which viewpoints can the supposed system of signs be simple? As already stated, including more than 400 signs, it was al least 10 times more complex of any graphic symbolic system in use in the same period in South and Central Asia.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

Sealings with impressions of square steatite seals were used for monitoring the closure and opening of vessels, doors (with cylindrical pegs, and most probably with more complicated wooden sliding locks), perhaps animal cages (see Frenez & Tosi forthcoming), wooden boxes or chests, bales packed with light materials and secured with string, and special firing containers or saggars. Single round flat sealings might have used as personal identity tokens. Beyond evident economic and administrative implications, such technology and functions are an expression of the overall concern that the Indus Civilization had with space control. This concern was a pervasive issue. It is powerfully reflected in the construction and management technology of the large urban systems of platforms, that protected the settlements from the rivers, improved security of the urban blocks, but also prevented the formation aside the urban lanes of the muddy and filthy surfaces one commonly see today along the roads of Pakistan and India's towns. It also appears in the related technologies of urban water management, so frequently mentioned in literature. High levels of urban space control, on the other hand, explain the rarity, in the major Indus excavated sites, of deposits and layers in primary context of deposition, after firings and other destructive events, that are more commonly encountered in other protohistoric cultures. Rubbish was systematically swept away.

This is probably the reason why sealings in the Indus sites are often found in secondary, displaced contexts. The absence (at least so far) of unambiguous evidence of hoards of used, opened and discarded stamped tags points to an administration carried out with contextual operations distinguished by a short intended time-span, and somehow permeated with an auto-referential logic. In two of the few archaeological contexts that with some confidence can be considered sub-primary (i.e. deposited with meaningful relationships with their previous setting) known for the Indus Civilization – the stoneware making workshop of Mohenjo-Daro and the so-called 'warehouse' of Lothal – clay sealings appeared respectively used in the management of craft production and, probably, in monitoring of a large group of packed goods in the ruins of a relatively large wooden building above a thick superstructure in mud bricks. Is this a coincidence? The careful excavations of Harappa have clearly showed that seals, clay sealings and inscribed objects, together with faience, terracotta and steatite miniature tablets, tend to be scattetered in discrete areas of the city, including walled sectors with substantial evidence of craft production and areas near the city gates, where raw materials and finished products could be more conveniently stored and moved around. That the use of steatite seals was somehow tied to management of craft production and trade of valuable goods, in the light of this archaeological evidence, is hardly questionable. Note also that there is no evidence, so far, that seals and sealings were used in centralized technologies of gathering and redistribution of primary agricultural production, nor seals and sealings have been found, after more than 80 years of excavations, in any context suggesting performance of rituals and the like.

I fully agree with Frenez & Tosi (forthcoming) when they state what follows:

> The socio-economic framework of the Indus Civilization was complicated by its polycentric organization of production, distribution and exchange, that were not administrated by a single central agency. The urban structure and the setting of Indus settlements confirm that they were not managed by a palace or a temple, in a monopoly system, like the urban settlements in Mesopotamia and Near East. It seems therefore impossible that the administrative organization of the Indus Civilization did not make continuous and systematic recourse to the record of commercial transactions and to the signature of contracts and

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

> proto-notarial deeds [...] it would be trivial thinking to debate about the possibility that an economic and productive system of this order of magnitude was developed and consolidated only on the base of spontaneity and mutual trustfulness.

Indus steatite stamp seals contain three orders of information: inscription, animalistic icon (realistic or imaginary) and often an object in front of the creature. Farmer & others, in their absolute and blind disregard for contexts, only consider the first one, while I believe that the two other elements, too, should have played an important role. Both at Harappa and Mohenjo-Daro, as well as in more important assemblages from other sites, the majority of the seals bears the image of the unicorn. The most complex administrative documents we have are clay sealings found at Mohenjo-daro, Harappa, Lothal, Kalibangan and perhaps more recently at Bagasra, a site deeply involved in various specialized craft activities (Sonawane et al. 2003), where unicorn seals were stamped one onto another, obliterating as a rule the common symbol of the unicorn, but carefully preserving, one below the other, the inscriptions – the expression and detailed recording of a chain of official responsibility.

Only *naive observers* could be tempted to equate the tiny Indus inscriptions left on clay tags in the closed and spaces of laboratories and storerooms with the large and bold religious symbols carved 16 centuries later on Mesopotamian stones for public vision. Only biased historians might seriously think and repeat that such developed and careful administration technologies used and registered, instead of writing, sequences of religious symbols soaked with ambiguity, with little and variable meaning, practically useless to memorize, and therefore open, when these documents had to be checked, to every kind of questioning and uncertainty (dummies, weren't they?).

A Lost Father...

The shadowy and fascinating masters of esoteric symbols envisaged by Farmer, Sproat & Witzel as the Indus elites are not a new entry on our hits. Although the authors never dare to directly touch the complex and sensitive point of the social organization of Indus societies, their personages closely resemble the chiefs and religious specialists that figured on the wonderful reconstructions of Indus major rituals in Fairservis (1986). It is clear that the overall reconstruction of the Indus society as loosely stratified clan society unified by symbols and strong ideological bonds provided by Walter Fairservis, Jr. in 30 years of research (1961, 1967, 1975, 1976, 1983, 1984, 1985, and particularly 1986, 1992 and 1994) would have fitted very conveniently the present views of the authors. Unfortunately, his theses were to a large extent derived from a systematic decipherment on which Walter Fairservis worked for a large part of his career, and that he tenaciously defended till his last years. It is with an obvious feeling of regret that Farmer & others state (n. 3, p. 20) that in the late 1960s Fairservis was almost ready to become the first major researcher to abandon a linguistic view of the script, but was unfortunately distracted by the misleading Dravidian hypothesis sponsored by Russian and Finnish linguists. Later (p. 25), they also credit him of having never supported the lost-manuscripts argument ([12]).

([12]) When I wrote these lines, I had not read Farmer 2003. This short paper is tear-filled, regretful for the great chance missed by Fairservis to become as clever as Farmer.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

I have a very vivid memory of Walter Fairservis. I was lucky enough to be his guest twice in Poughkeepsie at the annual meeting hold by the Society for Harappan Studies. It was a very informal gathering of Indus experts that drank lot of beer and, following a subtle paradox of North American culture, while pretending they were freely talking about their projects, played a lot of underground politics. We were housed in his exquisite compound, surrounded by pine trees, 2 hours drive from New York. From the windows, we were told, sometimes one could spot bears roaming in the wood. His library was filled with old leather-bound volumes, and we knew that he was also a writer of plays for Broadway, and that his daughters could perform at home in their own theatre. To me, southern European and leftist, Fairservis was the somehow scary personification of the power and the cultural authority of rich American elites. When he spoke to you and thought on his own at the same time, as he used to do, his look reminded me of the white-hooded American eagle: I loved him. I do not believe a single line of his decipherment, nor that The Indus Civilization was a clusters of bulls herders' chiefdoms. But his lectures on pre-dynastic Egypt and his reading of Narmer tablet were extremely influential to me, and his papers on the cultural ecology of Mohenjo-Daro, the relevance of bovines in Indus economies, his concept of extensive agricultural systems in the 3rd millennium B.C. were, I believe, powerful insights. Fairservis was the first one, 20 years before any other, to suspect that the Indus Civilization collapsed (or devolved) also because of the rice and millet cultivators along the south-eastern boundary of its sphere. In the same years, everybody was contented with the much less imaginative and colonialist thesis advanced by Mortimer Wheeler about the Aryan invaders from the north (completely dismissed by later research). Walter Fairservis was a unique mixture of genius, deep archaeological insight and naivety ([13]); and as the author(s) immediately found out, quite an uncomfortable father for everybody.

...and More Regained Stepfathers

Let us go back to Farmer, Sproat and Witzel and take now their statement that Indus cities cannot match the evolution of proto-urban city-states in Mesoamerica, whose cities '[...] *dwarfed* those of the Indus valley in size and sophistication' (p. 47). Sorry, another wrong statement. Similar views, while revealing the usual denigratory attitude ([14]), only depend upon the authors' poor understanding of the material contingencies of the archaeology of the Indus Civilization. Mohenjo-Daro, in the second half of the 3rd millennium B.C., came to extend across an area of about 400 ha, mostly covered by thickly packed private dwellings (data from the German-Italian survey of the 1980s and later discoveries in the present bed of the Indus river), involving an estimated population of

([13]) Differently from the authors, I use this term also in its positive values. Fairservis had a powerful, unaffected approach to archaeology, and felt continuous wonder and love for the prehistory of the Subcontinent. His personal trip through the Indus Civilization always reminded me the wonderful lives of some characters by Nikolaj Sem'novic Leskov.

([14]) Such comparisons, in this context, are totally meaningless, and I developed them here exclusively to show that the range of Indus urbanization has nothing to envy to urbanization processes in other cases of early state evolution.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

40,000-100,000 inhabitants (for these calculations, among others, I used the perhaps conservative rates in Sumner 1979). The population of Tikal, in the Late Classic around 600 A.D. (i.e., let us not forget, 3000 years later) may have included from 60,000 to 90,000 persons (Hester 2000: 888). Teotihuacan at its peak had a comparable (for some authors) or larger (according to others) population. The Citadel of Mohenjo-Daro is a colossal podium (about 200-210 × 350 × 15 m or more in height), possibly amounting around 800,000 cubic m or more of mud bricks and silt and lined with continuous, thick revetments in fired bricks, while the triangular massive platform under the HR insula might well be formed by not less than 200,000-250,000 cubic m of the same materials (Cucarzi 1987; Leonardi 1988). The whole system of platforms supporting Mohenjo-Daro and sheltering it from the reach of one of the world's mightiest river, including the Citadel and Lower Town, might amount to a total value not too distant from the volume of Khufu's pyramid at Giza, even if the construction of the substructures was not strictly contemporary. The platform under the Palace of Palenque, to push the comparison onward, includes about 80.000 cubic m of fill, ten times less Mohenjo-Daro's Citadel. Although other Mesoamerican buildings might have had a comparable size, or even, sometimes, more imposing dimensions, known examples of Indus monumental architecture show that the emphatic expression 'dwarfed' in this context is unrealistic and openly due to misinformed prejudice. This for the size. Coming to 'sophistication', this other rhetoric expression, largely abused, too, in archaeological writing, refers in most cases to loosely or expediently defined degrees of complexity and supposed refinement in material culture. If similar meaningless comparisons are pursued, the highly refined pyrotechnology of Indus craftspeople (copper, precious metals, glassy materials, heating of carnelian, fired bricks) would peak well above the average technologies of early Mesoamerican states.

The authors, I believe, have no need to support their arguments by exploiting old archaeological commonplaces that are largely dismissed by current research, even if they are still mindlessly repeated or copied in a parrot-like fashion even in prestigious contexts (compare their remarks at p. 46 and the box in Lawler 2004: 2027). We are lectured, maybe for the hundredth time, that the Indus Civilization is distinguished by the lack of monumental architecture, when we have colossal platforms supporting huge tanks and palace-like buildings with dozens of rooms, inner courtyards and showy facades with stone columns; by the lack of 'large-scale bureaucratic organization' – meaning that there are no central archives – when we might be in front of a pervasive standardized bureaucracy operating on crafts and trade with capillar efficiency at many levels of control across whole cities; by the lack of 'large temples', when we could simply deal with our own inability to recognize religious buildings or compounds (and as if the sometimes unbelievable waste of wealth in Sumer for temples and royal funerals were mandatory conditions of progress or cultural superiority. Who told us so?).

For what the lack of 'massive standing armies' is concerned, I suppose this other fancy and arbitrary assumption, as usual, depends upon the absence of artworks depicting the royal militaristic propaganda encountered in Egypt and Mesopotamia. While the military technology available to the Indus rulers, to the eyes of recent analysts, may appear as efficient and advanced as its western counterparts (Cork, unpubl.), everybody is free to decide if the propagandistic show of organized and divinely inspired use of violence in Eannatum's monument for the conquest of a bunch of fields is a sign of strength or, in contrast, of inner weakness. (Like many others, Lawler, in the *Science* paper, also refers to a lack of

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

fortifications, when the mounds of both Mohenjo-Daro and Harappa are surrounded by km of mud and fired brick walls with gates and bastions; to lack of clear evidence of social stratification – just because it is not commonly realized that Indus urban centers are *in toto* elite seats, and in the Indus gold and copper wealth was not destroyed in graves, but carefully preserved to be traded and inherited along family lines ([15]); and to the absence of sculptures, as if not making large statues and differing in this respect from Egypt and Sumer were another deadly historical sin).

Now, let us leave aside, for the moment, fortifications, introduced in Indus archaeology by the strong will of the army-addicted Sir Mortimer Wheeler (I refer to a personal communication by Prof. A.H. Dani. Left by Wheeler on the south-eastern corner of the Citadel of Mohenjo-Daro, he was told by his boss 'When I come back, I want to see the defences'). The above described diminutive view of the Indus Civilization, groundless and mainly defined, as usual, by 'lacks' and 'absences' of western traits, instead of an objective evaluation of its inner features, starts to acquire more recognizable contours. Add the last ingredient, the lack of evolution promptly identified by Farmer & co-workers across the 6 centuries of use of the script ([16]) (see p. 33 and below), even before thinking to a minimally credible diachronic study, and the soup is ready. But it tastes bad. The historical picture of the Indus Civilization they cooked and try to pass off as a formidable new achievement is a poor clone of that sponsored by the archaeological establishment in the 1950s and traditionally repeated for decades, from specialist papers to popular journals and books aimed at the general public.

Unnecessarily Abusive Language

In the paper, moreover, there is a revealing use of language. I find disturbing, for example, the use of quite abused rhetorical expressions such as 'claim, claims, to claim' whenever the ideas of the opponents are referred to, thus 'subtly' conveying the impression that such views are not serious (only at p. 21 these words repeat themselves not less than 10 times, *pace* the pursuing of a reasonably palatable English language. I have used it only twice!). And consider me, if you like, a Indo-Pakistani radical, or an Indus fundamentalist (I would like that), but the paper to me is undeniably drenched with tones of contempt for one of the key moments of the long trajectory of social evolution of the Subcontinent. Despise slips here and there through the lines. Thus, a miniature inscription on a potsherd from Harappa is '[...] a *crude* knock-off' from another class of artefacts (p. 24); although the authors deny that standard statistical tests may easily reveal whether or note a corpus of inscriptions encodes language (see above), 'he Indus system fails that test *miserably*' (p. 28, n. 14, my italics); the authors confidently write that the Indus sign system '[...] was not even evolving in linguistic directions after at least 600 years of use' (p. 33: dummies, weren't

([15]) See also the contribution of craft studies to the study of Indus early urban social stratification, in Kenoyer 1999.

([16]) The authors say that the Indus system was 'open', and not rigidly defined, but only to account for the possibility of the inclusion in the late periods of other vague symbols – and this would only have increased the loose and scarcely semantic nature they envision in the script.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

they?), and if the system was purely logographic its '[...] semantic range would be smaller than that of a typical three-year-old child, or even of chimpanzees taught to sign words in the laboratory' (pp. 33-34) (the authors might have imagined how offensive a comparison to monkeys, although admittedly indirect, might sound to the people and colleagues of the Subcontinent: was it really necessary? What does this last animalistic remark add to their arguments?) ([17]). Furthermore, Indus symbols '[...] may have been copied for centuries and in later eras [...] with little understanding of their original sense' and one should not 'attach too narrow a sense to any one Indus symbol or inscription' (p. 43).

It should be for this reason that the authors avoid the issue of a systematic decoding of the symbolic system they envisage in the Indus signs. Simply, given their idea of the scarce coherence of the signs' semantics, to them such a systematic effort would be worthless (but more about this later). Nonetheless, like many other decipherers in Europe, North America and in the Subcontinent, they felt confident enough (or could not resist the temptation) to write that they can easily understand the meaning of some symbols. For example, there is little doubt that the board inscription at Dholavira refers to a solar cult, as the sequence repeats a 'sun/power symbol' (p. 36). The authors might already have realized that they have joined a very crowded company.

...and the Collapse Melts Down

Well, sometimes you need outsiders to make a meaningful progress (see what Yuri Knozorov did for Mayan studies); in other cases, outsiders just fail (see what Knozorov did in the field of Indus studies). The Indus script, for the reasons listed above, is a particularly tough playground, and failures are routine. In summary, without unexpected enlightenment on the context and function of the ancient forms of communication to which the Indus system belonged, we cannot assume that brevity of its inscriptions alone indicates a non-phonetic character. The abandonment of the lost-manuscript dream does not minimally affect the reasonable hypothesis that the Indus system was an efficient early script. Its

([17]) Somebody should also tell Steve Farmer that his Italian friends that organized his lecture at the University of Bologna did not grant him a very good service. The Italian title of the talk, as still advertised in his site, was *Harappiani analfabeti*, presumably a translation of 'Illiterate Harappans'. Now, *Harappiani* sounds as odd in Italian as 'Harappians' would in English: it should have been *Harappani*, unless Farmer is willing to suggest that we should also start speaking of the 'Romians' and the 'Romian' civilization. It may have been a learned borrowing from the French form. *Harappiani*, anyhow demonstrates the lack of familiarity of the organizers with Indus archaeology and conveys a non-scientific, *amateur* like style that, given the pile of fantastic archaeology recurrently published on the subject, is not welcome. (At any rate, I prefer using terms like Indus people, Indus society, Indus Civilization). Secondly, in Italian the term *analfabeta*, while obviously indicating somebody who ignores the characters of the alphabet, in a country where till few decades ago a lot of the adult population could not write and read has retained an offensive meaning. (Furthermore, to strategically avoid writing would not be equal to ignoring the writing used in your own society). In my country, *analfabeta* is used as a frequent insult for gross, uneducated persons. Perhaps Farmer, as he did in other occasions, purposefully selected offensive language to impress his audience; or perhaps his translators thought it was funny, I don't know. But the title of his speech could have been more intelligent.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

distinctive dominance of high frequency signs is shared by other scripts of the 3rd millennium B.C. When we match the Indus signs with contemporary and contiguous systems of non-linguistic symbols, we see that the Indus script is not comparable, because its signs are on average 10 times more numerous or more. The Dilmun seals, in fact, demonstrate that a highly developed iconographic and symbolic apparatus, capable at the same time of expressing individual identities of the bearers, needs only 40-45 signs, and not 400. Even more explicit might be the case of southern Turkmenian figurines, where religious symbolism might have used only 6 basic symbols. Directionality in the signs sequences and their positional regularities, aspects of the Indus system that Farmer & co-workers try to de-emphasize, do hint to a script, also because these features (with the possible exception of pottery decoration, that anyhow is easily distinguished from writing) cannot be easily identified in contemporary non-linguistic systems. Furthermore, while these systems (and particularly those more elaborated from a formal and iconographic viewpoint) are highly redundant in that they make frequent use of duplication and serial repetition of the same images, the Indus script generally is not, although repetition of signs in the same inscriptions is present (in various forms), albeit in limited fashion. In some of these cases, there is no real reason to suppose that signs appearing with some form of graphic regularity are odd meaningless decoration. Comparisons of the Indus script with other writing systems (Luwian and Egyptian) proposed by Farmer & co-workers are biased by the lack of detailed information and archaeological evaluation of the relative contexts. These comparisons do not appear very relevant, although it is clear that the relatively low sign-repetition rates in single inscriptions should be considered (and hopefully, in the future, explained) as a peculiar feature of the Indus script. The comparison with the Near Eastern symbols of the 2nd-1st millennium B.C. is anachronistic and forced, biased again by a poor understanding of contexts and functions, as well as by a pervasive western-oriented prejudice (in spite of a total difference in material, context, form and function the Indus signs must be identical to the Assyrian-Babylonian emblems: no possibility of difference is provided for). The occurrence of rare or unique signs, as admitted by the authors too, is not a valid argument against phoneticism; actually, the archaeological evidence shows that unique signs might be even rarer in part of the non-linguistic systems here considered than in true scripts.

Besides its faults, the paper is constructed by repeatedly advancing hypotheses and sometimes wild speculation presented as serious scientific evidence. Leaving aside self-praising and the recurrent arbitrary identification in the Indus signs of symbols of deities, Farmer & co-workers, finding themselves on shaky ground, auto-referentially support their hypotheses with the pretended overall coherence of their own previous statements (pp. 37-38) or by referring, like in the case of their discovery of a 'sun/power symbol', to 'other evidence' (but what this revealing evidence should be, we are not told: see caption to fig. 6, p. 35). In other cases, pure guess is passed off as facts that, we are assured 'may be reasonably anticipated' by them (as in the caption to fig. 10, p. 41). I believe that such cases of lack of consistency have more to do with the authors' absolute certainty of being right, and with the urgency of skipping boring details, than with poor methodology.

Finally, if the authors' basic arguments may be thus criticized and often dismissed, their historical-archaeological inferences on the Indus Civilization should be openly rejected. Many of their statements – already fully criticized in this text – are based upon outdated information and biased judgment, and the reconstruction they propose as new discoveries are just a selection and resurrection of old interpretations once advanced more or less

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

implicitly by Stuart Piggott, Mortimer Wheeler, Walter Fairservis and others, and largely abandoned by current archaeological research as wrong or useless.

In order to understand the confidence of the authors' statements you have to consider their positive belief to have really made the final breakthrough – and in this respect their contribution is quite similar to those of the many enthusiastic would-be decipherers that piled on our desks hundreds of useless papers. I actually think that the most provocative aspect of the paper is the boldness of the authors' conclusions, in front of an evident lack of familiarity with the archaeological frame of the Indus civilization. I see no acceptable scientific demonstration of the non-scriptural nature of the Indus sign system; therefore, I see no collapse of such 'thesis'.

The Indus Civilization was 'diverse', as stated by the authors (p. 48), not because it lacked writing, but because it made of writing a limited use that we, after more of a century of efforts, blinded as we are by our western and uniformitarian bias, cannot even conjecture. The authors might confidently classify me too, in their words, as 'deeply committed to the script thesis' (see p. 37, n. 23), as this were a dangerous illness. Even if I do not believe it, there is the theoretical chance that the authors are at least partially right (i.e., that the purely symbolic component in the script has a larger role than originally assumed; and/or that the logographic component was more important than previously expected). Farmer and his colleagues should be credited, at least, as having raised some interesting points, and resurrected a debate that in the last two decades was definitely single-sided (for example, I confess that I had never paid serious attention before to the rarity of sign repetitions in the seals' sequences) ([18]).

The authors would like to throw the ball to their opponents, asking them to refute their views by providing a sound decipherment in linguistic terms. But they have raised the problem, proposing a different interpretation and the first readings, and they have to provide a demonstration of their thesis by interpreting and explaining to us the symbolic sequences following the equivalent of their condition 4. (as stated at p. 48). Those of us that are 'deeply committed to the script thesis', and forcefully content themselves with the present state of relative uncertainty, burn with expectation for the new exciting results (but for the moment even Farmer & others will admit that their deities on vessels and seals and the solar cult advertised at Dholavira did not cost them such an impressive outburst of imagination).

([18]) I only would like to stress that they have been preceded. One year before the publication of 'The Collapse...' this journal published a paper by Benille Priyanka entitled 'New Iconographic Evidence for the Religious Nature of Indus Seals and Inscriptions' where the Indus signs are interpreted as a system of non-scriptural standardized symbols. Priyanka's paper was internally coherent, and even if I personally disagreed totally with his interpretations, I recommended its publication. His approach, instead of beforehand assumptions and questionable statistics, was a comparison between sequences of signs and iconographic depictions, which in some cases seem to have a good match. Priyanka wrote: '[...] it is possible that the script would not yield a language or a phonetic decipherment [...] the decipherment itself will be the accurate understanding of the iconography of signs as standardized imagery and the Indus inscriptions as religious/ritualistic narratives [...]' (2003: 61). These are exactly the same conclusions of Farmer and his colleagues; but while the Americans flatly beforehand assume that in the signs there anyhow is little meaning, and therefore skip without the minimum embarrassment the issue of a validation of their thesis, for Priyanka even his symbols and icons might and should be interpreted.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

At the end, let me go back to the title. The authors would have been more honest if their paper would have been entitled something like 'The very idea of the Indus elites in the second half of the 3rd millennium B.C. being partially literate drives us crazy', or 'Our personal, deep anger at the idea of the Indus symbols expressing standardized sounds'. This is how, at least, the paper is perceived by a relatively distant reader. This, too, is not that new. The still unexplained power of Indus archaeology to foment the most extreme views and fierce individual passions, often and still on the basis of poorly organized bodies of data, knows no cultural or social boundaries, and will never cease to generate printed paper (and now infesting the web).

APPENDIX

Some constructive proposals for the development of studies on the Indus script:

1. Through the specialized archaeological congresses (for example South Asian Archaeology in Europe, major meetings in India and Pakistan, or the Madison conference in the States) let us identify a committee of no more than 5-6 'sages' from different countries, having an intimate knowledge of Indus material culture, archaeological contexts and inscriptions, and possibly not too deeply attached to one or another pre-constructed view (I know that this is probably the most difficult part, also thanks to the sharp tones of papers like Farmer, Sproat & Witzel's 'Collapse'). These 'sages' should finally decide how many different signs there are in the script, and how many signs, in contrast, should be considered variations and/or unique cases. This, in the future, would avoid at the ritual litany on this enduring problem we always have to read at the beginning of every paper on the subject.

2. On the basis of such a widely accepted list, we should create a computer program enabling everybody to easily write with Indus fonts in a PC Word environment, as well as, in the cases of new signs, to propose and create new entries (to be accepted by the committee). The acceptance of new signs should follow pre-established rules. Thus the publication of the inscriptions would become standardized and immediately useful to everybody, and data on inscriptions will be freely exchanged among scholars of different countries and continents. On this basis we should also support Parpola's hard efforts at building a central, standardized archive of Indus inscriptions with clear, easily accessible information and a careful evaluation of the inscriptions' overall contexts (see below).

3. Through the same committee and the same archaeological meetings, we should start international research on palaeography and formal development of the Indus system through time. Thanks to the exceptionally well constructed chronological frame of Harappa, and its many correspondences in the stratigraphy of Nausharo (Mehrgarh) we should be able to understand (hopefully, also on less easily seriated material from previous excavations) how far, and how many signs changed through time, thus understanding how many variations are diachronic, and how many synchronic. The best way of doing this would be to allot to promising students in different countries several MA and PhD theses, following a co-ordinated strategy. Such studies should be made on the archaeological materials, and not on published photographs or designs.

4. Such palaeographic studies should be carried out taking into account in a systematic fashion the material, functional and spatial variability of the inscriptions. Thus, we should search for recurrent variations in the signs' forms across contexts such as sites and regions, supporting material and technique of inscription, and context of occurrence (e.g., seals, miniature tablets, copper tools and other types). For example, inscription on seals should be strictly evaluated together with seal impressions on clay or sealings

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

and with terracotta miniature tablets moulded from seals, and not – at least at the beginning – with faience tablets or inscriptions on pottery. Inscriptions on metal tools should be considered as a separate corpus, and possible numerals in this medium should be later compared with possible numerals on seals and faience tablets. Inscriptions on pottery should carefully distinguish between pre- and post-firing cases (and even in this case direct access to the objects is mandatory). Post-firing inscriptions on lips of vessels should be matched with post-firing lip inscriptions, and those from the Indus core area should be compared, in a second moment, with similar lip inscriptions from peripheral areas; later, lip inscriptions should be matched with shoulder ones; and so on. This will inescapably create, at the beginning, very fragmented small-scale contexts, perhaps unfit for the immediate requirements of large-scale counts and easy statistics. Nonetheless, first one needs an accurate level of analysis before attempting any synthesis, and such levels have been never been attained by Indus script studies. It would be a long investigation, further complicated by the need to monitor how variations in such sub-contexts would be reflected or not in the course of time. The whole question needs to be dissected and re-combined at a level of 'accurate and intricate detail'. But without meaningful analytical contexts, no matter how limited and contextual, we might continue forever to discuss and write with little gain, and experts' counts will remain forever meaningless. Perhaps this is the right time to drop the Call of the Great Discovery and more humbly start a new phase of large-scale organization of the data. In the words of Stephen Jay Gould (2003: 42):

> In standard scientific practice, when tests of a favored hypothesis have failed, and one is beating one's head against a proverbial wall, the best strategy for reclaiming a fruitful path must lie in the empirical record, particularly in scrutinizing basic data for hints of a pattern that might lead to a different hypothesis.

REFERENCES

Ali, T., ed., (1994-95) *Excavations in the Gomal Valley. Rehman Dheri Report No. 2. Ancient Pakistan*, 10 (Special Issue).

Baines, J. & J. Málek (1985) *Atlante dell'Antico Egitto*. Novara. (Originally published as *Atlas of Ancient Egypt*, Oxford 1980).

Biscione, R. & M.G. Bulgarelli (1983) Painted Geometrical Decoration on the Shahr-i Sokhta Buff Ware: Approach to a Systematic Classification. In M. Tosi, ed., *Prehistoric Sistān*, 1. RepMem, XIX, pp. 211-64. Rome.

Cork, E. (unpubl.) Warfare in the Mature Harappan Period. Paper presented at the XVIIth International Conference on South Asian Archaeology, Bonn, July 7th-11th 2003.

Cucarzi, M. (1987) A Model of Morphogenesis for Mohenjodaro. In M. Jansen & G. Urban, eds., *Interim Reports*, 2., pp. 79-90. Aachen.

Damerow, P. & R. Englund (1989) *Excavations at Tepe Yahya, Iran, 1967-1975*. Volume II, *The Proto-Elamite Texts from Tepe Yahya. The American School of Prehistoric Research Bullettin*, 39.

Dossiers d'Archeologie (2003) Jiroft, Fabuleuse découverte en Iran. N° 287, Octobre 2003.

Durrani, F.A. (1981) Rehman Dheri and the Birth of Civilization in Pakistan. *Institute of Archaeology Bulletin*, 18, pp. 191-207.

Fairservis, W.A. (1961) *The Harappan Civilization: New Evidence and More Theory*. American Museum Novitates, 2302. New York.

Fairservis, W.A. (1983) The Script of the Indus Valley Civilization. *Scientific American*, 284, 3, pp. 58-66.

Fairservis, W.A. (1984) Harappan Civilization According to Its Writing. In B. Allchin, ed., *South Asian Archaeology 1981*, pp. 154-60. Cambridge.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

Fairservis, W.A. (1985) Sir Mortimer Wheeler and I: Archaeological Methodologies in the Indo-Iranian Borderlands. In G. Gnoli & L. Lanciotti, eds., *Orientalia Josephi Tucci memoriae dicata*. SOR, LVI, 1, pp. 343-54. Roma.
Fairservis, W.A. (1986) Cattle and the Harappan Chiefdoms of the Indus Valley. *Expedition*, 28, 2, pp. 43-50.
Fairservis, W.A. (1992) *The Harappan Civilization and Its Writing: A Model for the Deciphrement of the Indus Script*. New Delhi.
Fairsevis, W.A. (1994) The Harappan Script: Is It Deciphered or Can It Be Deciphered? In J.M. Kenoyer, ed., *From Sumer to Meluhha: Contributions to the Archaeology of South and West Asia in Memory of George F. Dales, Jr.* Wisconsin Archaeological Reports, 3, pp. 173-78. Madison.
Fairservis, W.A. Jr. (1967) *The Origin, Character, and Decline of an Early Civilization*. American Museum Novitates, 2302. New York.
Fairservis, W.A. Jr. (1975) *The Roots of Ancient India*. Chicago.
Fairservis, W.A. Jr. (1976) *Excavations at the Harappan Site of Allahdino. The Seals and Other Inscribed Material*. New York.
Farmer, S. (2003) Walter Fairservis and Indus Symbols. Downloaded from the site http://www.safarmer.com/indus/fairservis.html.
Franke-Vogt, U. (1991) *Die Glyptik Aus Mohenjo-Daro: Uniformitat und Variabilitat in der Induskultur, Untersuchungen zur Typologie, Ikonographie und Raumlichen Verteilung*. 2 vols. Mainz.
Frenez, D. & M. Tosi (forthcoming) The Lothal Sealings: Records from an Indus Civilization Town at the Eastern End of the Maritime Trade across the Arabian Sea. In *Studies Presented to Enrica Fiandra*.
Gould, S.J. (2003) *I Have Landed. Splashes and Reflections in Natural History*. London.
Gyselen, R. & W. Lerouge (1983) Project for a Descriptive Analysis of the Geometrical Ornaments on the Shahr-i Sokhta II-III pottery. In M. Tosi, ed., *Prehistoric Sistān*, 1. RepMem, XIX, pp. 265-302. Rome.
Hakemi, A. (1976) *Ecriture Pictographique Découverte dans les Fouilles de Shahdad*. Teheran.
Halim, M.A. & M. Vidale (1984) Kilns, Bangles and Coated Vessels: Ceramic Production in Closed Containers at Moenjodaro. In M. Jansen & G. Urban, eds., *Interim Reports*, 1, pp. 63-97. Aachen.
Hester, T.R. (2000) Il processo di urbanizzazione in Mesoamerica. In Istituto della Enciclopedia Italiana, *Il Mondo dell'Archeologia*, pp. 887-88. Roma.
Kenoyer, J.M. (1998) *Ancient Cities of the Indus Valley Civilization*. Oxford.
Kenoyer, J.M. (1999) Wealth and Socio-economic Hierarchies of the Indus Valley Civilization. In J. Richards & M. Van Buren, eds., *Order, Legitimacy and Wealth in Early States*, pp. 90-112. Cambridge.
Kjaerum, P. (1983) *Failaka/Dilmun. The Second Millennium Settlements*, 1.1. *The Stamp and Cylinder Seals*. Aarhus.
Lawler, A. (2004) The Indus Script – Write or Wrong? *Science*, 306, pp. 2026-29.
Leonardi, G. (1988) *Moenjodaro: From Surface Evaluation to Ground Testing. Interim Reports*, 3. Aachen-Roma.
Liverani, M. (2004) *Le Civiltà Mesopotamiche*. Torino.
Mahadevan, I. (1977) *The Indus Script: Texts, Concordance, and Tables*. Calcutta-Delhi.
Masson, V.M. & V.I. Sarianidi (1972) *Central Asia. Turkmenia before the Achemenids*. Southampton.
Méry, S. (2000) *Les Céramiques d'Oman et l'Asie Moyenne*. Paris.
Oppenheim, L.A. (1980) *L'antica Mesopotamia. Ritratto di una civiltà scomparsa*. Roma. (1st ed., *Ancient Mesopotamia. Portrait of a Dead Civilization*, Chicago 1964).
Parpola, A. (1994) Harappan Inscriptions. An Analytical Catalogue of the Indus Inscriptions from the Near East. In P. Mortensen, ed., *Qala'at al-Bahrein*, 1. *The Northern City Wall and the Islamic Fortress*, pp. 304-492. Aarhus.
Potts, D. (1981) The Potter's Marks of Tepe Yahya. *Paléorient*, 7, 1, pp. 107-22.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions

Pracchia, S. (1984) Preliminary Analysis of the Shahr-i Sokhta II Buff Ware Painted Figuration: Some Observations for a Systematic Classification. In B. Allchin, ed., *South Asian Archaeology 1981*, pp. 98-107. Cambridge.
Priyanka, B. (2003) New Iconographic Evidence for the Religious Nature of Indus Seals and Inscriptions. *EW*, 53, 1-4, pp. 31-66.
Quivron, G. (1980) Les marques incisées sur le poteries de Mehrgarh au Baluchistan, du milieu de 4e millenaire à la premiere moitié du 3e millenaire. *Paléorient*, 6, pp. 269-80.
Quivron, G. (1997) Incised and Painted Marks on the Pottery of Mehrgarh and Nausharo-Baluchistan. In R. Allchin & B. Allchin, eds., *South Asian Archaeology 1995*, I, pp. 45-62. New Delhi-Calcutta.
Rissmann, P. (1989) The Organization of Seal Production in the Harappan Civilization. In J.M. Kenoyer, ed., *Old Problems and New Perspectives in the Archaeology of South Asia*. Wisconsin Archaeological Reports, 2, pp. 159-70. Madison.
Salvatori, S. & M. Vidale (1997) *Shahr-i Sokhta 1975-1978: Central Quarters Excavations. Preliminary Report*. RepMem, Series Minor, I. Rome.
Sharif, M. (1990) Some New Seals from Mohenjo-Daro and the Evidence of Seal-Making. *Lahore Museum Bullettin*, 3, 1, pp. 15-18.
Sonawane, V.H., P. Ajiitprasad, K.K. Bhan, K. Krishnan, S. Prathapachandran, Abhijit Majumdar, Ajita K. Patel & Java Menon (2003) Excavations at Bagasra – 1996-2003: A Preliminary Report. *Man and Environment*, XXVII, 2, pp. 21-50.
Sumner, W.M. (1979) Estimating Population by Analogy: An Example. In K. Kramer, ed., *Ethnoarchaeology. Implications of Ethnography for Archaeology*, pp. 164-74. New York.
Tosi, M. (1983) Development, Continuity and Cultural Change in the Stratigraphical Sequence of Shahr-i Sokhta. In M. Tosi, ed., *Prehistoric Sistān*, 1. RepMem, XIX, pp. 127-80. Rome.
Tosi, M. (1991) The Indus Civilization beyond the Indian Subcontinent. In M. Jansen., M. Mulloy & G. Urban, eds., *Forgotten Cities on the Indus*, pp. 111-28. Mainz.
Vidale, M. (1987a) The Paste Plaques and Cylinders of Chanhu-Daro: A Descriptive Report. *AION*, 47, 1, pp. 57-66.
Vidale, M. (1987b) Some Observations and Conjectures on a Group of Steatite-debitage Concentrations on the Surface of Moenjodaro. *AION*, 47, 2, pp. 113-29.
Vidale, M. (1989a) A Steatite-cutting Atelier on the Surface of Moenjodaro. *AION*, 49, 1, pp. 29-51.
Vidale, M. (1989b) Specialized Producers and Urban Elites: On the Role of Craft Production in Mature Harappan Urban Contexts. In J.M. Kenoyer, ed., *Old Problems and New Perspectives in the Archaeology of South Asia*, pp. 171-81. Madison.
Vidale, M. (1990), Study of the Moneer South East Area: A Complex Industrial Site of Moenjodaro. *EW*, 40, 1-4, pp. 301-14.
Vidale, M. (1993) Bracciali in gres dell'età del Bronzo. *Le Scienze*, 300, pp. 46-57.
Vidale, M. (1995) *Viaggio intorno alla mia ciotola*. *AION*, 55, 3, suppl. 84.
Vidale, M. (2000) *The Archaeology of Indus Crafts. Indus Craftspeople and Why We Study Them*. RepMem, Series Minor, IV. Rome.
Vidale, M. (2005) The Short-horned Bull on the Indus Seals: A Symbol of the Families in the Western Trade? In U. Franke-Vogt & H.-J. Weisshaar, eds., *South Asian Archaeology 2003*, pp. 147-58. Bonn.
Vidale, M. (forthcoming) Some Archaeological Afterthoughts concerning Lothal and Mohenjo-Daro, to be presented at the International Conference of South Asian Archaeology. London 2005.
Winn, S.S.M. (2004) *The Old European Script. Further Evidence*. Downloaded from the site http://www.prehistory.it/ftp/winn.htm.

This content downloaded from 128.111.121.42 on Fri, 30 Oct 2015 00:38:55 UTC
All use subject to JSTOR Terms and Conditions